Robert Dover.

THE

BORDERLAND OF SCIENCE.

[*Reprinted from the 'Cornhill Magazine.'*]

THE

BORDERLAND OF SCIENCE:

A SERIES OF FAMILIAR DISSERTATIONS
ON STARS, PLANETS, AND METEORS; SUN AND MOON;
EARTHQUAKES; FLYING-MACHINES; COAL; GAMBLING;
COINCIDENCES; GHOSTS; &c.

BY RICHARD A. PROCTOR, B.A. (CAMBRIDGE),
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Carelessly I roam
As through a wide museum—from whose stores
A casual rarity is singled out
And has its brief perusal, then gives way
To others, all supplanted in their turn.
WORDSWORTH.

WITH A PORTRAIT OF THE AUTHOR.

LONDON:
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PREFACE.

In the two series of my 'Light Science for Leisure Hours, several essays from the *Cornhill Magazine* were included; and in one or two reviews of those works remarks appeared implying that every essay I had written in serial publications was included in those volumes. This is so far from being the case, that more than three-fourths of my essays in serials have not been published in a collected form, and will not be. I have made it my rule to collect only a few essays, carefully selected from amongst a large number. The present volume, while apparently an exception to this rule, is not so in reality, and in fact affords evidence of its operation. It is true that I here present all but some seven or eight of those of my essays in the *Cornhill Magazine* which have not hitherto been published in a collected form. But, in reality, essays on Popular Science intended for the *Cornhill Magazine* are subjected to so careful an editorial process of 'selection and rejection' as can lead only to the 'survival of the fittest,' so that such

essays as appear in that magazine may be regarded as the selected work of the author to whom they are due. And the fact that, after already drawing from this source, so large a volume as the present can be formed from my *Cornhill* essays, though seven or eight papers are for various reasons left unused, will serve to show that in former volumes of essays I have not too exhaustively drawn upon the resources available to me.

I have little to remark by way of preface on the contents of the present volume. The opening essay touches towards its close upon some considerations respecting the Star-depths, which have been based on a long series of original researches. These, as well as my independent investigation of the circumstances of the approaching transits of Venus, will, in a very short time, be published in a volume entitled 'The Universe and the Coming Transits.'

The second and third essays are more fanciful, perhaps, than befits even the 'Borderland of Science.' The reader must regard them as merely indicating (so far as physical phenomena are concerned) such circumstances as would, in my opinion, be recognised by a being capable of voyaging through the solar system.

It appears to me that the considerations urged in the essay upon Coal, respecting the exhaustion of the supply, are sounder than those against which they are advanced. In fact, although the essay is dated October 1872, when

the present high price of coals had been already reached, I may yet claim to have anticipated the recent check to the process of exhaustion, since similar views were indicated in a paper specially devoted to that portion of the subject, which appeared in the *St. Pauls Magazine* for November 1871, and forms one of the essays in the second edition of the first series of my 'Light Science for Leisure Hours.'

Some of the anticipations in the essay on Flying Machines have been realised since the essay was written by the construction of aerial instruments depending for their support on the rapid motion of plane surfaces.

A singular illustration of the mischievous effects which may result from foolish superstitions, such as are dealt with in the essay on that subject, was afforded very recently. The ship *Adela*, loaded with stores for our countrymen engaged in the Ashantee war, and ready to start on Friday, September 19, was delayed (according to the *Daily News*) in consequence of the sailors' unwillingness to start on a Friday.

RICHARD A. PROCTOR.

LONDON: *October* 1873.

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THE BORDERLAND OF SCIENCE.

THE HERSCHELS AND THE STAR-DEPTHS.

THE astronomer whose loss science has lately had to lament, brought to a close, thirty-five years ago, the most wonderful series of researches yet recorded in the history of astronomy. For more than half a century those researches had been in progress, and during all that time the astronomer engaged upon the work had been recognized as the first astronomer of his time. From 1780 to 1822 Sir W. Herschel was engaged in surveying the star-depths: after 1822 the researches were carried on by Sir John Herschel, second to no astronomer of our day, nor to any observational astronomer the world has yet produced save his father alone.

It is well that the real nature of the work accomplished by the Herschels should be recognized, for otherwise just honour will not be done to their memory.

It is amazing, indeed, that it should now be necessary to correct mistaken impressions on the subject; yet there can be no question that few know rightly what are the real claims of the Herschels to the admiration of the world and to the gratitude of astronomers. It was but necessary to peruse the obituary notices which appeared during the week following Sir John Herschel's death, to find how little the work of the Herschels has been appreciated. In those notices we commonly saw the labours of the elder Herschel associated—as was fit—with the work of the son, and yet the real end and aim of those labours and of Sir John Herschel's, altogether missed by the biographer.

The real work of the Herschels—that end to which all their labours were directed—was the survey of those regions of space which lie beyond the range of the unaided vision. Other work they did which well deserves attention. The elder Herschel, in particular, has left papers describing observations of the planets, careful studies of the sun's surface, and researches into a variety of other subjects of interest. But all the work thus recorded was regarded by him rather as affording practice whereby he might acquire a mastery over his instruments, than as work to which he cared to devote his whole powers. Even the discovery of a planet travelling outside the path of Saturn, although this discovery is commonly regarded as the most noteworthy achievement of Herschel's life, was in reality but an almost accidental result of his real work among the star-depths. It was, in truth, such an accident as

he may be said to have rendered a certainty. No man can apply the powers of telescopes larger than any before constructed, to scrutinize, as he did, every portion of the celestial depths, without being rewarded before long by some such discovery: and it was well, in many respects, that Sir W. Herschel was thus rewarded, because the recognition which his labours thenceforth received, undoubtedly facilitated the prosecution of his researches. But those labours had another and a nobler end than the mere discovery of unknown planets. He never prosecuted them for a single hour without discovering multitudes of unknown orbs far mightier than the massive bulk of Uranus. These discoveries passed unrecorded, save numerically, so many were they; but they tended to the solution of the noblest problem which men have yet attempted to master. That the true end of Sir W. Herschel's labours was the mastery of this problem, must be obvious to any one who will be at the pains to examine those volumes of the *Philosophical Transactions* in which his researches are recorded; but he has also plainly told us his purpose in continually applying more and more powerful telescopes to the survey of the celestial depths. 'A knowledge of the construction of the heavens,' he wrote in 1811, 'has always been the ultimate object of my observations.'

I do not propose here to enter into the details of the various processes of inquiry in which the active mind of Sir W. Herschel led him to engage while he was attempting to solve the secret of the star depths. I

wish rather to present results than to consider methods.

Yet the first of Herschel's researches was so full of interest, and led to a result so strange, that it will be well briefly to consider its purport.

When Herschel began his labours he hoped not merely to determine the general arrangement of the stars throughout the spaces around us, but also to ascertain the real architecture (if one may so speak) of the stellar system. To this end it was necessary that the distances of the stars should be ascertained; and, accordingly, one of the first subjects to which Herschel applied his powers was the amazingly difficult one of measuring the stars' distances. A method of extreme ingenuity, but also (as was commonly the case with Herschel's devices) of extreme simplicity, suggested itself to his mind. Of course the only real means of determining any star's distance must depend upon the effects of the earth's motion round the sun. If the earth were at rest we should see the star always in a certain direction, but how far off it lay in that direction we could never know. It is because the earth takes up different positions, so that we see a star at different times in different directions, that we have a means of estimating the star's distance. But the earth's path, despite the 180,000,000 of miles of its diameter, is so minute compared with the spaces which separate our sun from the nearest stars, that astronomers had despaired in Herschel's time of measuring the change of seeming direction due to the earth's motion. An

observer might at one time notice that his telescope had to be pointed in a certain direction to bear on a particular star, while six months later (when the earth would be 180,000,000 of miles from the spot she had occupied before) the observer might try to note whether his telescope required to be pointed in some slightly different direction to bear on the star. But in the meantime the stand of the telescope might have been slightly moved, as by the sinking of a pier, or even by changes due to greater warmth or cold. The air might not act precisely in the same way on the rays from the star. The observer's own powers might have varied, or rather, these and other like changes must inevitably take place to some extent, however slight; and it had begun to be known in Sir W. Herschel's time that the slightest possible error of the kind would suffice to render any attempts at measurement ineffective.

Herschel at once suggested a means of overcoming all these difficulties. What we want, he reasoned, is to tell towards what point of the heavens a star seems to lie, at different seasons, and the nearer the star the more it would seem to shift. A star so far off as not to be visible without a powerful telescope will not seem to shift at all; for it must probably be twenty or thirty times farther away than the bright stars, and we know that even these shift so slightly that we cannot be sure they shift at all. What is to prevent us, then, from regarding one of these faint and therefore very distant stars as a sort of index-point from which to measure the minute excursions of some bright star close by it

on the heavens? If we do this it will not matter whether our observatory or our telescope have slightly shifted, whether the air acts more or less strongly in bending the rays of light from the star, and so on. For now, we are no longer concerned in trying to find the absolute place of the star upon the heavens, but in noting how it seems to be placed with regard to a neighbouring star, an inquiry which can be in no way affected by these difficulties.

Now Herschel had repeatedly noticed faint stars very close by bright ones. There were some instances in which the faint star was so minute and so close by the larger one, that it required one of his most powerful telescopes to see the small star at all as an object distinct from the larger one. Cases such as this obviously promised to afford very satisfactory information about star distances. The very faint orb must lie at an enormous distance beyond the bright one—so, at least, Herschel believed,—while a fortunate chance seemed to have placed the two orbs so nearly in the same direction that the least displacement of the brighter orb, on account of the earth's motion, must necessarily be made apparent.

But the careful study of many such cases brought only disappointment, so far as Herschel's main object was concerned. There was absolutely no trace, in any instance examined by him, of that seeming vibratory motion of the brighter orb, year after year, which Herschel had hoped to recognize. The conviction grew gradually upon him that there had been a flaw in his

reasoning. And inquiring where that flaw could be, he presently saw that his assumption of the relatively enormous distance of the faint star must be ill-founded. Then he went farther, beginning to believe that the fainter and the brighter star lay at the same distance,—in other words, that they formed a physically associated pair. This view—since firmly established by his own labours and his son's—changed altogether the meaning of the lessons taught by the stars. For hitherto men had believed that the stars are distributed through space in such sort as to be independent of each other. A few thoughtful men—as Wright, Kant, Lambert, and Mitchel,—had ventured to express doubts as to the justice of this view; and Mitchel, indeed, had by the mere force of abstract reasoning, anticipated the very conclusion to which observation had now led Sir W. Herschel. But it is in the nature of men, of scientific men as well as others, to turn an almost deaf ear to abstract reasoning, however sound, and to note only what is established by observation; so that, as we have said, the general belief among astronomers had been that the stars are distributed throughout space, not in systems, but singly.

In the meantime Sir W. Herschel had turned his attention to the general architecture of the heavens. He had sought in particular to determine the figure of that vast scheme of orbs of which our sun is a member. The method he employed for this purpose was simple in the extreme.

Let it be supposed that the system of stars has

definite limits, and that within those limits stars, resembling our sun, are distributed with a certain general uniformity. Then it is quite obvious that, if we look towards those parts of the star-system where the limits are farthest away, we shall see the greatest number of stars, supposing always that our vision reaches to the limits of the system in such direction. So that if we have but a sufficiently powerful telescope to pierce to the very boundary of the star-system, and if we always use the same telescope so as to make sure that we are always dealing with the same range of the heavens, all we need do in order to determine the shape of the star-system is to count the number of stars seen in different directions. Where there are few stars the boundary of the star-system must be relatively near; where many stars are seen the boundary must be far away.

Perhaps not a single reader of these pages needs to be told that it was by applying this method—which he called star-gauging—that Sir W. Herschel was led to the belief that the system of stars is shaped like a cloven flat disc. And I suppose every reader is familiar also with the picture which is introduced into all our books of astronomy to illustrate this theory of the star-system. I have before me, as I write, Sir W. Herschel's own drawing, in the volume of the *Philosophical Transactions for the Year* 1785, and, after a careful reperusal of the accompanying paper, I cannot wonder that a theory so noble in itself, and presented with the simple grandeur of diction which distinguishes Sir. W. Herschel's astronomical speculations, should have en-

gaged the earnest attention of astronomers, and should again and again have been referred to or quoted in astronomical treatises. Nor can I greatly wonder that Sir W. Herschel's own confidence should have been shared by those who have presented his theory. 'I have now viewed and gauged the Milky Way,' he says, 'in almost every direction, and find it composed of stars whose number, by the account of these gauges, constantly increases and decreases in proportion to its apparent brightness to the naked eye. That this shining zone is a most extensive stratum of stars of various sizes admits no longer of the least doubt, and that our sun is actually one of the heavenly bodies belonging to it is as evident.'

When to this I add that Sir John Herschel, gauging the depths of the southern heavens, was led to precisely the same conclusions as to the general structure of the Milky Way, it seems impossible not to regard the theory so often presented in our books as involving the definite conclusions of the Herschels respecting the scheme of the fixed stars. Nor is it necessary to add that conclusions thus accepted by the greatest authorities in stellar astronomy that have ever lived, must be such as few students of astronomy would care to call in question.

It will therefore surprise many to be told that, as a matter of fact, seventeen years had not passed after the elder Herschel had enunciated that theory which has been so often presented in astronomical treatises, and which his own son seems always to have regarded as

established, before Sir W. Herschel abandoned the theory as untenable. In the picture and paper of 1785 we find our sun one of innumerable stars, not all equal, indeed, nor spread with mathematical uniformity, but still all comparable with each other in magnitude and distributed with a general approach to uniformity. In 1802 we find Sir W. Herschel regarding our sun as one of a set of stars which he called insulated stars, and the Milky Way as composed of stars wholly different in their nature and arrangement. I quote his own words lest the reader should be disposed to doubt the very possibility that in so many treatises a theory should have been assigned to Sir W. Herschel which he had himself rejected. After saying that our sun, magnificent as its system is, must yet be regarded as only a single individual of the species he denotes by the term 'insulated star,' he presently proceeds:—'To this may be added that the stars we consider as insulated are also surrounded by a magnificent collection of innumerable stars called the Milky Way. For though our sun, and all the stars we see, may truly be said to be in the plane of the Milky Way, yet I am now convinced by a long inspection and continued examination of it, that the Milky Way itself consists of stars very differently scattered from those which are immediately about us.' And a few pages further on the very principle of the method of star-gauging, and the conclusions as to the shape of the Milky Way, are thus unmistakeably called in question. 'In my sweeps of the heavens,' says Herschel, 'it has been fully ascertained

that the brightness of the Milky Way arises only from stars; and that their compression increases in proportion to the brightness of the Milky Way. We may, indeed, partly ascribe the increase both of brightness and of apparent compression, to a greater depth of the space which contains these stars; but this will equally tend to show their clustering condition; for, since the increase of brightness is gradual, the space containing the clustering stars must tend to a spherical form, if the gradual increase of brightness is to be explained by the situation of the stars.'

But we cannot rightly understand either the theory which Sir W. Herschel thus abandoned,* or that by which it was replaced, without considering his researches into objects quite different from the fixed stars.

In ancient times astronomers had noticed five spots on the heavens where a cloudy sort of light could be recognised. These spots they had called 'cloudy stars.' But not very long after the invention of the telescope, several more of these star-cloudlets began to be recognised. Lacaille discovered forty-five in the

* The eminent German astronomer, Struve, thus writes respecting Herschel's change of view:—'Remarquons d'abord que, dès 1802, il n'est plus question de la figure de la Voie Lactée dans les recherches de Herschel. Elle n'est plus une strate limitée, car elle est insondable, et il devient impossible d'en embrasser la totalité.' And again, 'Nous parvenons donc au résultat, peut-être inattendu, mais incontestable, que le système de Herschel, énoncé en 1785, sur l'arrangement de la Voie Lactée, s'écroule de toutes parts, d'après les recherches ultérieures de l'auteur; *et que Herschel luimême l'a entièrement abandonné.*' Yet an assertion to this effect, made by the present writer in the presence of the Royal Astronomical Society, two years since, was received with obvious signs of incredulity.

southern heavens, and Messier, the comet-seeker, made a list of no less than 103. The star-cloudlets or nebulæ known when Sir W. Herschel began his researches, amounted to less than 150. In the year 1786 that astronomer began his contributions to the list of known nebulæ by sending a catalogue of no less than 1,000 of these objects to the Royal Society. Three years later he sent in a list of yet another thousand nebulæ; and in 1802 (when he was sixty-four years old) another list, containing 500 of these objects. In other words, during sixteen years this indefatigable observer noted the places of more than sixteen times as many of these celestial cloudlets as all preceding observers had been able to record. Sir John Herschel, having proposed to himself the task of completing at a southern station the survey of the heavens which his father had commenced, thought it necessary to prepare himself for the work by re-surveying the northern heavens. While thus engaged he discovered 500 nebulæ which had escaped his father's notice. Then, proceeding to the Cape of Good Hope, he examined those parts of the heavens which had been invisible from his father's northerly observatory, and in 1847 communicated a list of 1,708 star-clouds discovered during the progress of this survey. In all, Sir John Herschel discovered no less than 2,208 nebulæ, his father having discovered 2,500. As the whole number of known nebulæ in our day amounts to but 5,200, it will be seen that more than nine out of every ten known nebulæ were discovered by the Herschels.

And here let us pause for a moment to endeavour to realize the fact that more than five thousand of these *clouds* exist within the range of telescopic vision. The number of stars visible to the unaided eye in the whole heavens is about five thousand—that is, on a dark and clear night average eyesight can recognize about 2,500 stars of different orders of brightness. Now suppose that all the stars were suddenly destroyed, the nebulæ alone being left, and that at the same time our powers of vision were suddenly increased to such an extent that we could see all objects visible in the telescopes with which the Herschels surveyed the heavens. Then we should see about as many faint cloud-like specks of light as would correspond to the number of stars we *now* see. And if, further, the defining powers of the Herschelian telescopes could be given to us, we should recognize in these cloud-like specks all the various orders into which the Herschels divided the nebulæ. Here we should see straggling clusters very little condensed in their central portions, there globular clusters so rich in stars as to shine with unspeakable glory—insomuch that it has been well remarked of some of them that no one who beholds them for the first time in a telescope of adequate power can refrain from a shout of rapture. In some regions the oval nebulæ, close set with stars or wholly irresolvable, would be seen, in others spiral and ring nebulæ, the strange forms of the 'dumb-bell nebula,' the 'crab nebula,' the 'key,' the 'flight of wild ducks,' nebulous stars, and the planetary nebulæ, shown under

the power of the great Rosse telescope as among the most fantastic of the celestial cloudlets. While lastly, long irregular streamers and wisps of cloudy light, seemingly shapeless and unintelligible, would be seen in those regions of the heavens where now are seen the constellations Orion and Argo, the Swan and the Archer.

It was these wonderful objects which led Sir W. Herschel to propound the noblest theory of the universe which the world had yet known, or rather (for Lambert and Kant had, in some respects, anticipated Herschel's theoretical considerations), the noblest theory which men had yet attempted to place on an observational basis. He recognized in many of these seeming cloudlets galaxies like our own, like that wonderful scheme of stars the glories of which he had himself laboured to make known to us. In fact, he called certain of these objects Milky Ways, remarking that many of them 'cannot well be less, and are probably much larger, than our own star-system; and being also extended, the inhabitants of the planets which attend the stars which compose them must likewise perceive the same phenomena [that we do]. For which reason these nebulæ may be called Milky Ways by way of distinction.'

This conception of more star-systems than the one of which our sun is a member is unspeakingly impressive. We are altogether unable, indeed, to form any adequate idea of the relations which we express easily enough in words. There are many ways of presenting the considerations dealt with by Sir W. Herschel, and yet

every one of these methods must be regarded as in many respects unsatisfactory. We may consider, on the one hand, the seeming minuteness of the distance separating the stars of a nebula from each other, and then endeavour to realize the fact that that distance, only just rendered appreciable by the magnifying power of the largest telescopes man can construct, is assuredly not less but probably exceeds many hundred-fold the distance separating our sun from the neighbouring suns—this last distance being so enormous that it has been calculated that the swiftly-travelling comets which visit us from the interstellar spaces cannot have occupied less than ten millions of years in traversing it. Or again, we may endeavour to picture to ourselves the vastness of the distances which must separate us from these outlying Milky Ways, when millions of such orbs as our own sun, though all shining at the same time within the field of view of a powerful telescope, yet present only the appearance of a faint milky light which the thinnest haze can blot from our view. Or lastly, and this, perhaps, affords the most striking means of indicating the grandeur of Herschel's conceptions, we may endeavour to picture the fact that this earth on which we live, and those companion orbs whereof many so largely exceed our earth in mass and volume—the solar system, in fine, which has so often been presented to our contemplation as in itself a sort of universe—would seem a mere point if viewed from the nearest fixed star, and yet that each point of the millions which make up the

milky light of a nebula must be regarded (if these conceptions of Sir W. Herschel be just) as the centre of a scheme as vast as the solar system, and possibly far vaster.

Another conception, even more overwhelming, is that of the distances separating these Milky Ways from each other. For vast as are the dimensions of the several Milky Ways, including our own, the distances separating one from another are far vaster—belonging, indeed, to a higher order of vastness altogether.

And here the question will suggest itself, What position (according to these views) does our own Milky Way bear among the others? We have already quoted Herschel's opinion as to the dimensions of our galaxy, which he supposed to be far surpassed by those of many other galaxies. But he also came to an opinion as to the relative *age* of our Milky Way, which cannot fail to strike the reader as singularly indicative of the daring originality of his mind. 'If it were possible,' he says, 'to distinguish between the parts of an indefinitely extended whole, the nebula we inhabit might be said to be one which has fewer marks of profound antiquity than the rest. To explain this idea perhaps more clearly, we should recollect that the condensation of clusters of stars has been ascribed to a gradual approach; and whoever reflects on the numbers of ages that must have passed before some of the clusters could be so far condensed as we find them at present, will not wonder if I ascribe a certain air of youth and vigour to many regions of our sidereal stratum.'

Sir John Herschel has also exhibited the relations of this theory of external Milky Ways, in passages of a striking nature. In one respect, indeed, he has passed even beyond the limits ranged over by his father's daring ideas, insomuch that while Sir W. Herschel spoke only of systems of Milky Ways, his son has urged the idea of systems of such systems, and has even suggested the possibility that some of the celestial cloudlets may belong to this higher order. 'To us,' he says, 'the material universe must be regarded as practically infinite, seeing that we can perceive no reason which can place any bounds to the further extension of that principle of systematic subordination which has already been traced to a certain extent. . . . . It by no means follows that all those objects which stand classed under the general designation of "nebulæ" or "clusters of stars," and of which the number already known amounts to upwards of five thousand, are objects of the same order. Among those dim and mysterious existences, which only a practised eye, aided by a powerful telescope, can pronounce to be something different from minute stars, may, for anything we can prove to the contrary, be included *systems of a higher order* than that which comprehends all our nebulæ (properly such), reduced by immensity of distance to the very last limit of visibility.'

But we must distinguish between that which is possible or even probable, and that which the astronomer has been able to demonstrate. If we examine the progress of Sir W. Herschel's researches into the

nebulæ, we find that side by side with that gradual but, in the end, complete change which we have already noted in his views respecting our own Milky Way, there was an equally gradual, and in the end, an equally complete change in his ideas respecting the greater number of the celestial cloudlets. Nor will it be difficult to recognise the way in which each change bore upon the other. Nay, it could readily be shown, if this were the place for a close analysis of Herschel's ideas, that the changes in his views (1) as to the nature of double stars; (2) as to the constitution of our star-system; and (3) as to the nature of the nebulæ,—were all part and parcel (perhaps unconsciously to himself) of a modification of the principle itself according to which he interpreted his observations.

It may be well, as I have already quoted what he wrote in 1802, when his ideas respecting the Milky Way underwent their most marked modification, to quote the remarks with which, in 1811, he introduced his modified views respecting the general constitution of the heavens. 'I find,' he says, 'that by arranging the nebulæ in a certain successive regular order, they may be viewed in a new light, and, if I am not mistaken, an examination of them will lead to consequences which cannot be indifferent to an inquiring mind. If it should be remarked that in this new arrangement I am not entirely consistent with what I have already in former papers said on the nature of some objects that have come under my observation, I must freely confess that by continuing my sweeps of the heavens, my

opinion of the arrangement of the stars and their magnitudes, and of some other particulars, has undergone a gradual change; and, indeed, when the novelty of the subject is considered, we cannot be surprised that many things, formerly taken for granted, should on examination prove to be different from what they were generally but incautiously supposed to be. For instance, an equal scattering of the stars may be admitted in certain calculations; but when we examine the Milky Way, or the closely-compressed clusters of stars, this supposed equality of scattering must be given up. We may also have surmised nebulæ to be no other than clusters of stars disguised by their very great distance, but a longer experience and better acquaintance with the nature of nebulæ will not allow a general admission of such a principle, although undoubtedly a cluster of stars *may* assume a nebulous appearance when it is too remote for us to discern the stars of which it is composed.

The new views respecting the constitution of the heavens, introduced in this paper, related chiefly to those nebulæ which, though otherwise conspicuous, yet when examined even under the highest powers of Sir W. Herschel's largest telescope, presented a milky appearance. He now for the first time expressed the opinion that such nebulæ do not consist of multitudes of stars, but of some self-luminous substance of exceeding tenuity. He recognised the existence of this luminous vapour amidst large tracts of the heavens; and he regarded it as certainly lying within the limits

of our galaxy, and forming, therefore, part and parcel of its constitution. Nay, more; he stated his belief, and brought strong evidence to show, that this vaporous *matter was the substance out of which the stars have* been made. He pointed to different milky nebulæ which seemed to belong to different stages of growth, from an exceedingly faint and altogether irregular nebulosity, to rounded nebulæ, nebulæ with faint centres, nebulæ with bright centres, nebulæ consisting almost wholly of a bright central light (the outer portion being scarcely discernible), and, finally, nebulous stars—this being the last recognizable stage in the progress to actual stars or suns.*

There is something singularly impressive in the ideas suggested by this theory, whether as respects extension in space or duration of time. Of course, in one respect, this new view of certain orders of nebulæ implied an enormous diminution of the estimated dimensions of these objects. Taking, for instance, the wonderful mass of nebulous light which seems to cling around the sword of the giant Orion, it will be obvious that if this object were supposed to lie far beyond the limits of our star-system, and to consist of countless millions of suns so far off as not to be separately discernible, the nebula would be an altogether more wonderful object than it becomes on the supposition that it lies within our galaxy, or even *nearer* (as Sir W. Herschel

* I purposely omit here any reference to Sir William Herschel's explanation of the so-called planetary nebulæ; because neither the explanation itself nor the objections to it would well admit of popular exposition, at least within the space here at my disposal.

believed) than the stars seemingly immersed in it. In reducing the distance of this object many hundreds of times, Herschel was reducing its vastness many millions of times. But then it is to be noted that in simply ceasing to view this particular nebula as a vast external system of suns, Herschel was by no means seeking to show that no such systems of suns exist outside our galaxy. On the contrary, all the arguments from analogy, on which he had founded his belief in external star-systems, remained unimpaired, as also did much of the observational evidence. And *now* Herschel was showing our galaxy as a much more wonderful scheme than it had hitherto been supposed to be. For, according to these new views, vast as has been the time during which our galaxy has been in existence, it has not yet completely formed itself into stars. Over vast regions belonging to it, enormous masses of nebulous matter are gradually condensing into stars,—single, double, or multiple. The imagination is wholly unable either to conceive the duration of the time-intervals which have been and will be occupied by these wonderful processes, or to picture the stupendous nature of those laboratories of our galaxy, in which its suns have had their genesis.

Nothing is more remarkable, perhaps, in the history of scientific theories than the circumstance that while Sir W. Herschel's theory of self-luminous vapour existing within the limits of the galaxy is very commonly spoken of, the actual fact that he thus anticipated one of the most remarkable discoveries of recent times,

seems almost wholly overlooked. Again and again, in books of astronomy and in scientific papers, Dr. Huggins's discovery that many of the nebulæ are vast agglomerations of glowing gas, is spoken of as strikingly opposed to the views of Sir W. Herschel. The circumstance is, indeed, of a piece with the fact to which we have already referred—that ideas respecting the Milky Way, which Herschel was the first to reject, are still presented as confidently as though they were the fruits of his matured experience.*

What was really overthrown by Dr. Huggins's discovery, was the opinion, which had been gradually gaining ground, that Sir W. Herschel had been mistaken. For instance, Professor Grant, in one of the finest works on astronomy which the last quarter of a century has produced, wrote thus: 'Notwithstanding the ingenuity of illustration and the incontestable force of reasoning by which Sir W. Herschel sought to establish his bold hypothesis, it has not received that confirmation from the labours of subsequent inquirers which is so remarkable in the case of many of the other speculations of that great astronomer. In fact, the greater the optical power of the telescope with which the heavens are surveyed, the more strongly do the results tend to produce the impression that all nebulæ are in reality vast aggregations of stars, which assume a nebulous aspect only because the telescope with which

* Well may Struve ask, 'Ne serait-il pas temps que l'Angleterre se décide à honorer la mémoire de son plus grand astronome, par une édition complète et systématique de ses œuvres?'

they are observed in each instance is not sufficiently powerful to resolve them into stars. Sir John Herschel himself, notwithstanding that tendency to reverence his father's dicta which has seemed so reprehensible to one biographer, was disposed to entertain the same opinion; for he says, "it may very reasonably be doubted whether there is any essential physical distinction between" clusters of stars and those nebulæ which his father regarded as composed of a shining nebulous fluid, and whether such distinction as there is "be anything else than one of degree, arising merely from the excessive minuteness and multitude of the stars, of which the latter, as compared with the former, consist."'*

But during Sir John Herschel's researches in the southern heavens, evidence of a very significant nature was obtained concerning this very question. I do not hesitate, indeed, to say that the facts now about to be described throw more light on the question of external Milky Ways than any which astronomical observation has yet revealed.

In the southern skies there are two strange patches

* Lest I should seem to dwell unduly here on the mistakes of men so eminent as Professor Grant and Sir John Herschel, I quote my own opinion as recorded in 1865, in my treatise on Saturn, published on the same subject. After defining 'Herschel's Nebular Theory,' I said, respecting it, that 'modern discoveries do not favour it. It appears probable that with sufficient telescopic power, all nebulæ would be resolvable into stars.' Scarcely had these words been published when I received from Dr. Huggins the account of the spectroscopic discovery that the Orion nebula, and several others, are composed of glowing gas.

of milky light which have long been known by sailors as the Magellanic Clouds, because Magellan was the first voyager who recorded their existence. Astronomers, however, usually call these objects the *Nubeculæ*. Both are nearly round, and their light, when they are viewed with the unaided eye, corresponds exactly with that of the Milky Way in regions of medium brightness.

We owe to Sir John Herschel the first systematic survey of these interesting objects. The result is full of interest. In one respect telescopic scrutiny shows that the Magellanic Clouds resemble the Milky Way in constitution; for scattered over both clouds are myriads of stars of all magnitudes from the eighth downwards. But also there are numbers of nebulæ within the limits of both clouds, whereas the ground of the Milky Way is singularly free from true nebulæ. Nor are the nebulæ in the Magellanic Clouds so spread that we can attribute their appearance within the limits of the clouds to accident, or judge their real position to be (conceivably) far out in space beyond the myriads of stars just referred to. On the contrary, the space all round both the Magellanic Clouds is singularly free as well from stars as from nebulæ. To use Sir John Herschel's own striking expression, 'the access to the Nubeculæ on all sides is through a desert.' No doubt, then, can remain that the nebulæ seen in the Magellanic Clouds are within the same region of space as the small stars seen along with them.

Now let the reader carefully note the significance of these facts. The reasoning by which that significance

is educed is exceedingly simple; but the result is of the utmost importance.

Each of the Magellanic Clouds, as we have said, is nearly round. Now when an object appears round, the most probable opinion we can form respecting the object's shape is that it is globular. An object which is not globular *may* appear circular, as for instance, an egg, a roller, or the like, looked at endwise, or a coin looked at in a direction square to its flat surfaces. But we know that if an egg, or a roller, or a coin, were held in a random position, the chances would be against that position being such that the egg, or roller, or coin would present its round aspect, so to speak. And clearly, therefore, if we know nothing about a certain object but that it appears round, we must accept as probable the belief that it is globular.* This conclusion, which would be justly arrived at in the

* We have an instance of this sort of reasoning in the case of the moon. We know nothing, certainly, about the shape of the moon regarded as a solid, for we only see her under one aspect. So far as abstract possibilities are concerned, the moon, as seen under certain aspects from Venus, might present the shape of an egg, or even of a diamond. Still we conclude that the moon is a globe, because she presents the aspect which a globe, and a globe only, presents in all positions. (Lately astronomers have indeed seen reason for questioning this conclusion; but my present argument is not affected by the circumstance.) But now let us conceive a case directly illustrating the argument dealt with above. Suppose a certain fruit of unknown nature is held in such a position, and at such a distance, that all we can recognise of its aspect is its seeming outline, and that this outline is round. We should regard it as probable that the fruit is globular. Now if a second specimen were similarly held up (in a random position) and seen to be also round, we should be very strongly confirmed in our opinion, and the mathematical theory of probabilities shows us that this natur-

case of one object, is much strengthened when two objects of the same general aspect, but quite independent of each other, *both* appear to be round. We cannot reasonably doubt, then, that the region of space occupied by each Magellanic Cloud approaches to the globular form.

But if the Magellanic Clouds are globular objects, we can tell the relative limits of distance between which all objects in either cloud must lie. To illustrate our meaning, let us take the sun's globe. One point of that globe is nearer to us than any other, and one point is farther away than any other. The nearest point is that which appears to lie at the centre of the solar disc, the farthest would appear to occupy exactly the same position, if the sun were a transparent globe. Now we can tell how much farther *relatively* the latter point is than the former, without at all considering the *actual* distance of the sun. The sun might be only a thousand miles away, or a thousand billions of miles, and yet the relative distances of these two points would be the same. As a matter of fact, if the distance of the nearest point of the sun's globe is called one hundred, then the distance of the farthest is slightly less than a hundred and one. Precisely the same reasoning applies to each of the Magellanic

ally deduced conclusion is a just one. For instance, suppose—to use our ordinary modes of expression—that the odds are three to one against an egg-shaped fruit appearing round (under such circumstances as are dealt with above), then the odds against two such egg-shaped fruits appearing round would be no less than fifteen to one.

Clouds, only the relative distances are not the same as in the sun's case, because the Magellanic Clouds both cover a much larger portion of the sky than the sun does. In the case of the larger Magellanic Cloud, it is easily shown that if the distance of the nearest part of that globe-shaped cluster be called *nine*, the distance of the farthest part must be about *ten*. In the case of the smaller, the distance of the farthest part is yet more nearly equal to that of the nearest part.

We have, then, this altogether unexpected result, that, so far as the nebulæ in the Magellanic Clouds are concerned, we have not to deal with galaxies external to our system, but with objects mixed up with stars of the eighth magnitude—that is, with stars which had always been regarded by astronomers as lying far nearer to us than the outskirts of the star-system. 'It must be taken as a demonstrated fact,' says Sir John Herschel, 'that stars of the seventh or eighth magnitude' (that is, stars only just beyond the limits of the unaided vision), 'and irresolvable nebulæ,' (that is, objects which had been supposed to lie hundreds of times farther away than the outermost bounds of our own star-system), 'may co-exist within limits of distance not differing in proportion more than as nine to ten, a conclusion which must inspire some degree of caution in admitting *as certain* many of the consequences which have been rather strongly dwelt upon' in the treatment of the elder Herschel's researches.

Now it may seem highly venturesome to press this conclusion more earnestly than Sir John Herschel himself

has seemed willing to do. Yet we must not forget that it was a peculiarity of Sir John Herschel's mode of dealing with such matters, that he did not press facts home very strongly. He had not, indeed, a firm grasp of facts. Again and again in his published works we find him reasoning in absolute forgetfulness—or as if in absolute forgetfulness—of facts he had already demonstrated or admitted. He differed in this most markedly from his father, who never once let go his grasp of a fact. Both these great men had a light hold of theories, but the elder Herschel had at the same time a vice-like hold of facts,—Sir John Herschel not unseldom let them slip through his fingers.

I therefore confidently urge the 'demonstrated fact' spoken of by Sir John Herschel, as 'a conclusion which must inspire' something more than 'caution in admitting' the consequences which had been supposed to flow from the elder Herschel's studies of such irresolvable nebulæ as he did not consider to be gaseous. Sir W. Herschel had judged that multitudes of these nebulæ must be external Milky Ways; the 'demonstrated fact' is that a large group of such nebulæ happening to be so placed that their distance (relatively to isolated stars) can be estimated, are *not* external galaxies, but much nearer to us than many parts of our own galaxy. In the only cases in which we *can* judge, these star-cloudlets are found *not* to be external star-systems; is not this a ground for something more than caution as to the theory that in the other cases, where we have no means of judging, such star-cloudlets *are certainly* external

star-systems? Take any really parallel case and the answer to this question will be obvious. Suppose a botanist had asserted his belief that all the plants presenting certain characteristic features were poisonous, no evidence beyond the existence of those features being at the time available, and that at length some person made actual experiment on ten or twelve orders of plants having such features, and found that they certainly were *not* poisonous—would not this demonstrated fact dispose entirely of the reasoning, however ingenious it might be, on which the general theory of the poisonous nature of such plants had been supposed to be established? Would it not be a fair inference that the untried orders were at least *probably* innocuous? And would it not be thought strange if a botanist, commenting on the discovery that all the as yet tried orders of plants having certain characteristics were innocuous, were to say, 'This demonstrated fact must inspire some degree of caution in admitting *as certain* the conclusion that the remaining orders of such plants are poisonous'? I yield to none in my respect for the great astronomer whose loss science is now deploring. I entertain most strongly the opinion that he was far the greatest astronomer of our time; but truth compels me to say that in his mode of dealing with demonstrated facts, and especially in this particular instance, he was, to say the least, not so happy as his father. He seems almost to have regretted to see certain questions pass beyond the field of controversy into the domain of the known.

But, after all, how aptly this 'demonstrated fact' of Sir John Herschel's fits in with the work of his father! When we note how the views of the elder Herschel had been gradually modified, and the course on which the progression of his theories had led him, we see that the fact discovered by the younger Herschel was only somewhat in advance of the point reached by the father, but lies strictly in the direction along which he had been progressing up to the very close of his career. Sir W. Herschel had modified his views about unequal double stars—concluding that the fainter orb is physically associated with the brighter one, instead of lying far beyond it. He had modified his views as to star-groups of various orders. He had given up the idea that our star-system can be gauged—regarding the great cloud-masses of the Milky Way as real clustering aggregations of stars, instead of depths extending far out into space and owing their seeming richness only to such extension. He had come to regard many star clusters as part and parcel of the Milky Way, and large numbers of nebulæ as vaporous masses lying far within its limits. It seems impossible to question how *he*, at least, would have regarded the discovery made by his son. He would have felt, I conceive, that so far as the evidence went, the sole remaining objects which could till then be regarded as external galaxies, must no longer be so regarded,—that *these*, like so many objects which he had himself dealt with, must be looked upon as among the wonders of our own star-depths. Nor do we think that in arriving at this conclusion, in making this

further advance along the road which he had already traversed so far, he would have judged that he was adopting views in any respect less wonderful or less awe-inspiring than those grand, yet mistaken, theories, in which hundreds of other Milky Ways had figured. On the contrary, he would have felt that in obtaining an enhanced estimate of the extent, variety, and vitality of our own star-system, we were at the same time being led to form nobler opinions as to the myriads of other star-systems which doubtless exist, though as yet no telescope has revealed them to our contemplation.

(From the *Cornhill Magazine* for July 1871.)

A VOYAGE TO THE SUN.

[ALTHOUGH the following narrative is related in the first person, it is not to be understood that the account was actually written by the voyager. The writer of these introductory lines does not deem it desirable to particularize the manner in which this account has reached him. He prefers to leave the reader to guess whether (like Cardan) the voyager who is responsible for the principal facts, saw, in a vision, what is here described; or whether 'the interiors of the spirit' were 'opened in him,' as chanced to Swedenborg, so that he could 'converse with spirits, not only those near our earth, but with those also who are near other

orbs;' or whether, like the author of the 'Neue Reisen in den Mond, in die Sonne, &c.,' he obtained his information through the agency of *clairvoyance*; or, lastly, whether spiritualistic communications from departed astronomers are here in question. According to the ideas which the readers of these lines may severally entertain respecting the manner in which such facts as are here described may have been ascertained, they will doubtless decide for themselves among these explanations, and others which may, for aught the writer knows, be available. Nay, there may even be some who may be disposed to regard the whole of what follows as a mere effort of imagination. For his own part the writer must be content to present, without comment or explanation, the information which has reached him; there are, indeed, some circumstances in the account which he could not explain if he would. It will be noticed that from time to time the narrator refers to explanatory communications having reference to the real nature of the voyage. These communications belong to the details, which it is not desirable to enter upon at present.]

Our voyage commenced shortly before noon on January 9, of the year 1872. As we started from the central part of London—or, to be more particular, from the rooms of the Astronomical Society in Somerset House,—our course was directed, in the first instance, towards a part of the sky lying southwards, and some sixteen degrees above the horizon. From what we have

already told you, you will understand that the earth's attraction did not in the least interfere with our progress. But atmospheric resistance was not altogether so imperceptible; and from time to time, notwithstanding our familiarity with all the astronomical details of our journey,—and X.'s special mastery of the laws to which we were to trust,—we found considerable inconvenience from the loaded state of the lower atmospheric strata. Although we were no longer subject to any physical inconveniences (indeed, our enterprise would otherwise have been impracticable), and although our powers of perception were greatly enhanced, yet the very circumstances which enabled us to exercise powers corresponding to those of the common senses, rendered the veil of mist and fog which surrounded us on all sides, as impenetrable to our vision (to use this word for want of a better) as to the eyesight of the Londoner.

Presently, however, we rose into a purer atmosphere. The sun,—the end and aim of our journey,—was seen in a clear sky, while below us the vast mass of cloud and fog which hung over London appeared like a wide sea, shining brilliantly under the sun's rays, and effectually concealing the great city from our view.

Our flight was now very rapid, and each moment becoming more so, as we reached rarer and rarer regions of the upper air. We noticed that the noise and hubbub of London seemed rapidly to subside into what appeared to us at the time as almost perfect stillness. And in passing I may confirm what Glaisher has

said respecting the voices which are heard to the greatest distance. For the shrill tones of women and children were heard from time to time, when the loudest tones of the male voice were altogether beyond our hearing. The sounds which we heard latest of all, however, were the occasional shrieks of railway-whistles, and (quite unexpectedly) a peculiarly shrill note produced by the beating of the sea-waves on the shore, which I do not remember to have observed under other circumstances. We noticed this as our onward course carried us past (though far above) the waters of the British Channel.

I forbear to speak of the aspect presented by the earth as our distance gradually increased; though, for my own part, my attention (at this part of our progress) was directed far more closely to the planet we were leaving than to the orb which we proposed to visit. X., on the other hand, absorbed (as you will readily believe) in the anticipation of the revelations about to be made respecting the sun, directed his sole attention to the contemplation of that luminary. Y., who accompanied us (as we have already informed you), rather *en amateur* than because of any profound interest which he takes in scientific investigations, appeared to be too much perplexed by the unexpected appearance of all the objects now in view to attend to any special features of the scene. He was in particular surprised at the rapidly increasing darkness of the sky in all directions, except where the sun's intense lustre still lit up a small circle of air all round his orb. Long

before we had reached the limits of the terrestrial atmosphere the stars began to shine at least as brilliantly as in ordinary moonlight; and when certain signs recognized by X. showed that we were very near the limits of the air, the stars were shining as splendidly all around as on the darkest and clearest night. At this time X. asked us to turn our attention to those parts of the sky which were most remote from the sun, in order that when we were actually beyond the terrestrial atmosphere, we might see at once the full glory of a scene which he had been contemplating for some time with unutterable wonder. I am, therefore, unable from my own experience to describe how the effects of atmospheric illumination in concealing the real splendour of the regions closely surrounding the sun had gradually diminished as we rose into rarer and yet rarer strata.

But while we were preparing for the surprise which X. had promised, a surprise of another kind awaited all of us. It had become clear that, although the tenuity of the air through which we were now passing was almost infinitely greater than the gaseous rarity produced in any experimental researches undertaken by men, we were yet approaching a definite boundary of the terrestrial atmosphere. None of us were prepared for the effects which were produced when that boundary was crossed. On a sudden the darkness of the heavens all round us increased a myriadfold, insomuch that the darkness of the blackest night seemed like midday by comparison. Yet I speak here only of the blackness

of the background on which the stars were shown; for the light of the stars as suddenly increased in an equal degree, while thousands of thousands of stars not before seen, in a moment leapt into view (I can use no other expression). The familiar constellations were there, but they seemed lost in the splendour of a thousand more wonderful constellations hitherto unrevealed, except ('as through a glass and darkly') to the telescopist. Each star of all these unnumbered thousands shone with its proper splendour, and yet each, as respects size, seemed to be the merest point of light. It would be utterly useless for me to attempt to describe the amazing beauty of the spectacle thus presented, or the infinite complexity of structure seen amidst the star-depths. We stayed for a while entranced by the sublime picture suddenly disclosed to us; and it was with difficulty that X. (even more enthusiastic, you remember, as a student of the stars than as one of our modern sun-worshippers) could be withdrawn from the contemplation of the wonderful display.

One other circumstance I must mention before describing the scene which we witnessed when the sun and sun-surrounding regions became the object of our study. I have spoken above of the silence which prevailed around us after we had reached a certain height above the earth. To our infinite amazement, we found, as we passed the limit of the atmosphere, that what we had regarded as silence,—nay, as an almost oppressive silence,—was only silence by comparison with the noise and tumult lower down. A sudden change from the

uproar of the fiercest battle to the stillness of the desert could not surpass in its effects the change which we experienced as we passed through the impalpable boundary of the earth's atmospheric envelope. What had seemed to us like an oppressive silence appeared now, by contrast, as the roar of a storm-beaten sea. We experienced for the first time the effects of absolute stillness. It is certain that Pythagoras was right when he spoke of the tumult which, in reality, surrounds us, though,

> Whilst this muddy vesture of decay
> Doth grossly close us in, we cannot hear it.

Yet as to the harmony of the spheres, he was mistaken; for even when the unnoticed but ever present mundane noises suddenly ceased, as we passed the limit of the earth's airy vesture, no sound betrayed the swift rush of the planets on their course round the sun. We were still close to the earth, the desert of Sahara lying now vertically beneath us at a distance of rather more than 400 miles, yet her onward rush at the rate of more than eighteen miles per second produced no sound which could be perceived, even amid the intense silence —the *black* silence, as X. called it— of interplanetary space.

And now, how shall I fitly describe the scene which was revealed to us as we directed our attention towards the sun? He was scarcely nearer to us—at least, not perceptibly nearer—than as commonly seen, and yet his aspect was altogether new. His orb was more brilliantly white than it appears when seen through the

air, but a close scrutiny revealed a diminution of brilliancy towards the edge of his disc, which, when fully recognised, presented him at once as the globe he really is. On this globe we could already distinguish the spots and those bright streaks which astronomers call *faculæ.* But it was not the aspect of his globe which attracted our wondering attention. We saw that globe surrounded with the most amazingly complex halo of glory. Close around the bright whiteness of the disc,—and shining far more beautiful, by contrast with that whiteness, than as seen against the black disc of the moon in total eclipses,—stood the coloured region called the sierra; *not red,* as we had expected to see it, but gleaming with a mixed lustre of pink and green, through which, from time to time, passed the most startlingly brilliant coruscations of orange and golden yellow light. Above this delicate circle of colour towered three tall prominences and upwards of thirty smaller ones. These, like the sierra, were not red, but beautifully variegated. We observed, however, that in parts of the prominences colours appeared which were not seen in the sierra—more particularly certain blue and purple points of light, which were charmingly contrasted with the orange and yellow flashes continually passing along the whole length of even the loftiest of these amazing objects. It was, however, worthy of notice that the prominences round different parts of the sun's orb presented very different appearances; for those near the sun's equatorial zone and opposite his polar regions

differed very little in their colour and degree of light from the sierra. They also presented shapes reminding us rather of clouds moving in a perturbed atmosphere, than of those tremendous processes of disturbance which astronomers have lately shown to be in progress in the sun. But opposite the spot zones, which were already unmistakably recognizable, the prominences presented a totally different appearance. They resembled jets of molten matter, intensely bright, and seemingly moving with immense velocity. One or two formed and vanished with amazing rapidity, as when in terrestrial conflagrations a flame leaps suddenly to a great height and presently disappears. Indeed, the whole extent of the two spot zones, so far as we could judge from our view of the region outside the bright solar disc, seemed to be in a state of intense electrical disturbance, since the illumination of the solar atmosphere above and around these zones appeared not only brighter than elsewhere, but was here subject also to continual changes of brightness. These changes, viewed from our great distance, did not, indeed, seem very rapid, yet, remembering the real vastness of the atmospheric regions, it was impossible not to recognize the fact that they implied the most intense activity in the solar regions beneath.

It was clear, even at the great distance at which we still were, that the solar atmosphere extends far above the loftiest of the coloured prominences. We could not yet distinguish the actual boundary of the atmosphere, though we entertained little question, after what we had

discovered in the case of the earth's atmosphere, that a real boundary exists to the gaseous envelope surrounding the sun. But we could perceive that a brightly luminous envelope extended to about twice the height of any prominence visible at the moment, and that the solar atmosphere extends and remains luminous to a far greater height than this more brilliant region. But the most amazing circumstance of all was this, that above even the faintest signs of an atmosphere, as well as through and amidst both the inner bright envelope and the fainter light surrounding it, there were the most complex sprays and streams and filaments of whitish light, here appearing as streamers, elsewhere as a network of bright streaks, and yet elsewhere clustered into aggregations, which I can compare to nothing so fitly (though the comparison may seem commonplace) as to hanks of glittering thread. All these streaks and sprays of light appeared to be perfectly white, and they only differed among themselves in this respect, that, whereas some appeared like fine streaks of a uniform silvery lustre, others seemed to shine with a curdled light. The faint light outside the glowing atmosphere surrounding the prominences was also whitish; but the glowing atmosphere itself shone with a light resembling that of the sierra, only not so brilliant. The pink and green lustre,—continually shifting, as it appeared to us, so that a region which had appeared pink at one time would shine a short time after with a greenish light,—caused us to compare the appearance of this bright region to that of mother-of-pearl. I suppose

that, at a moderate computation, this glowing envelope must extend to a height of about a quarter of a million of miles from the sun; while from where we were we could trace the fainter light of the surrounding atmosphere to a distance of about half a million miles from the sun's surface. As for the white streaks and streamers, they were too irregularly spread and too complicated in their structure for us to form a clear opinion as to their extension. Moreover, it was obvious that their real extension was greater than we could at present perceive, for they gradually became less and less distinct at a greater and greater distance from the sun, and finally became imperceptible, though obviously extending farther than we could trace them.

We had passed more than two millions of miles beyond the moon's orbit — our progress being now exceedingly rapid — when we encountered a meteor-stream, which appeared to be of great extent. We had already noticed the passage past us of many single meteors, which seemed to cross our path in all directions. But the members of the meteor-system now encountered were all travelling nearly in the same direction, coming from below (if we may so describe the portion of space lying south of the general level in which the planets travel) slantingly upwards, and nearing the sun, though not on a course which would carry them within several millions of miles of his globe. This meteor-system is not one of those which our earth encounters; nor could X. — who, as you know, has closely studied this subject — recall the path of any

comet which travels along the course which the meteors of this system were pursuing.

We paused to study, with not a little interest, a system which belongs to a class of cosmical objects playing, as would appear, a most important part in the economy of the universe. The members of this meteor family were small—few of them exceeding a few inches in diameter—and separated by relatively enormous distances. Except in the case of a few sets of two or three or more of these bodies, which evidently formed subordinate schemes, we could not perceive any instances in which any meteor was separated by less than a hundred miles from the nearest of its fellows, insomuch that it was impossible for us to perceive more than a very few of these objects at a time. More commonly, indeed, two or three thousand miles separated each meteor from its immediate neighbours. Yet the actual number of the bodies forming this system must be enormous, for we found that the system extended in the direction in which we were travelling, for no less than a million and a half of miles, and its longitudinal extension—that is, its extension measured along the orbit of the system—must be far more enormous, even if the system does not form a closed ring, as in other cases known to terrestrial astronomers. It is, however, somewhat unlikely that this can be the case; for we observed that the meteors were travelling at the rate of about twenty-six miles per second, which implies (so, at least, X. asserted) that the path of these meteors is a very eccentric one, extending farther into

space than the paths of the most distant known members of the solar system.

Most of the meteors were rounded, though few were perfectly globular; some, however, appeared to be quite irregular in shape. We were interested (and Y. was not a little amused) to observe that most of the meteors were rotating as steadily as though they were of planetary importance: the sets of meteors, also, which we have already referred to, were circling round each other with exemplary gravity. A strange circumstance, truly, that those peculiarities of planetary motion, which we are accustomed to associate with the existence of living creatures (whose requirements these movements so importantly subserve) should thus be simulated by the minute orbs which wander to all appearance uselessly through space!

After passing this interesting region, and travelling more than three million miles farther on our course towards the sun, we noticed for the first time that a change had passed over the appearance of the sun's atmosphere and the surrounding regions. The radial streamers respecting which astronomers have so long been in doubt had come into view in the most unmistakeable manner. We could trace them from the very border of the sun's globe; across the inner glowing atmosphere as well as the outer and more faintly illuminated region; and beyond that region to distances which we judged to vary from some seven or eight millions of miles opposite the solar spot zones to about two millions and a half opposite the polar and equa-

torial regions of his globe. Yet it must not be inferred that the radiated glory now visible around the sun was, strictly speaking, four-cornered. There was a general tendency to the four-cornered or trapezoidal form, but the apparent figure of the light was gapped and striated in an irregular manner, suggesting that the real shape of the portion of space through which these radial gleams extended was far from simple. We could not trace any actual outline of the coronal glory; so far as we could judge, it merged itself gradually into a faintly illuminated background of light, which, as we could now perceive, surrounded the sun to a vast distance on all sides, but with an obviously increased extension opposite the sun's equatorial regions.

The most remarkable circumstance, however, in the radial aspect now presented by the outer corona, was the fact that it had undoubtedly not been so well marked—even if it had existed at all—only a short time before. There could, indeed, be no mistake about the matter; some strange process of change had taken place whereby the coronal region had become thus marvellously striated. The same process of change had caused all parts of the solar atmosphere, excepting only the sierra, to glow more resplendently. But the streaks and sprays of faint white light remained unchanged, as well in shape as in lustre and colour. They appeared now by contrast somewhat fainter than they had been; and, of course, owing to our having drawn nearer to them, they appeared somewhat larger: but we agreed that, in reality, no appre-

ciable change whatever had affected these mysterious objects.

As it seemed not unlikely that we should shortly witness farther changes in the radiated glory, which we could not but regard as probably auroral in its nature, it appeared desirable to X. that we should endeavour to time the continuance of the aspect now presented. A sufficiently accurate measurement of time seemed likely to be obtained by noting the moon's motion. The earth and moon were now far behind us, appearing as two planets of great splendour, and close together. The apparent diameter of the earth was about a sixth of that commonly presented by the moon; while the moon, which was approaching the earth (in appearance) from the left, showed a diameter equal to about a fourth of the earth's. Both seemed appreciably 'full,' that is, shone with full circular discs, the moon seeming to shine with a somewhat fainter degree of luminosity. This was, no doubt, due to the inferior reflective qualities of her surface, or rather, to the superior reflective power of clouds in the earth's atmosphere. For we could distinctly perceive that the middle part of the earth's disc, occupied at the time by the Atlantic Ocean, showed a band of whitish light, north and south of which the ocean presented a purplish colour much darker than we should have expected, and certainly not shining with more light than the general surface of the moon. The ice-covered regions round the southern pole could be plainly recognised by the brilliant whiteness of the light they reflected; and

all the appearances suggested that this part of the earth is occupied by an ice-covered continent.

Not to digress further, however, I return to the consideration of the method by which X. proposed to time any solar changes. The moon was now, as I have said, very close to the earth in appearance, and slightly below or south of the earth, speaking always with reference to the general level of the paths on which the planets travel—on which level, as I have said, we judged it well to pursue our course. At the moment we could see that the distance separating the moon and earth was equal (in appearance) to about six times the apparent diameter of the earth; and X.'s long experience enabled him to form an exact estimate on this point. It was only necessary, therefore, to compare this distance with that noted subsequently, as occasion might arise, to form a tolerably exact estimate of the time which should then have elapsed. For it will be understood that, placed as we were, we could quite readily recognize the relatively rapid motions of the moon on her course round the earth. And in passing we may mention how strange it appeared to us to see the earth, so long known to us as a body to be contrasted with the celestial orbs, now taking her place as a planet among the stars. There, not far from Jupiter (whom she very much outshone at the time), among the familiar though now enhanced splendours of the constellation Gemini, shone our earth and her satellite,—a double planet, and next to the sun himself the most beautiful object in the heavens.

During the next ten million miles of our progress we passed the neighbourhood of several meteor systems, actually traversing three, whereof two were far more important, so far as we could judge, than the one already described. It was worthy of notice also that the members of all these systems travelled much more swiftly than the meteors formerly seen.

But what appeared to us a most remarkable circumstance was this, that as we drew nearer towards the sun, these meteor systems became more numerous and more important, while we could recognize many objects resembling comets in their general structure (only they had no tails), but much smaller, insomuch that many of them appeared to be only a few hundred miles in diameter. They were in a general sense round, and became more numerous as we proceeded; while in several instances we observed that they appeared in groups. It would seem from this that multitudes of comets, too small to be discerned by any telescopes yet made, exist within the confines of the solar system; but whether these are the remains of larger comets, or have an independent cosmical existence, it is difficult to determine. Before we reached the orbit of Venus (now shining very brilliantly on the left of the sun, and through our own motion passing rapidly from Aquarius to Pisces) these objects began to appear in countless numbers, with obvious signs of an increased condensation in the sun's neighbourhood. We could perceive that for the most part they were followed by flights of meteors, individually

minute, but more closely packed (so to speak) than the meteor systems near our own earth. We began to suspect that this unexpected wealth of cosmical matter in the sun's neighbourhood might supply the explanation of those interlacing streaks and sprays and hanks of whitish light to which reference has already been made.

When we were about half-way between the paths of Venus and Mercury, we for the first time noticed a diminution in the distinctness of those auroral radiations which had first made their appearance when we were but some six millions of miles from the earth. It seemed as though the glowing streamers were slowly fading from view, in the same way that streamers of an auroral display wane in splendour even as we watch them. In a short time we could no longer distinguish the radiations, the solar atmosphere resuming the appearance it had presented when we first observed it. Unfortunately we were unable to estimate the length of time during which the radiated appearance had continued visible, for we were now much too far from the earth to estimate with any degree of accuracy the amount by which the moon had advanced on her course. But though X.'s ingenious plan had thus failed to afford an exact estimate, we could still infer from the aspect of the earth and moon, that some three hours of common time had passed since the radial streamers appeared.

It seems difficult to understand how the phenomenon we had witnessed could be otherwise regarded than as

a solar aurora. How the electrical action causing such an aurora is excited, seems open to question: though the facts to be presently described suggest a probable explanation. But after what we had now seen, I had myself very little doubt that electricity is the main cause of the phenomenon.

Passing Mercury (some twenty millions of miles on our right as we crossed his orbit) we began to draw so close towards the sun, that many of the features shown by good telescopes could be clearly recognized. His spots already presented a striking appearance; but we were most interested at this stage of our progress by the aspect of the coloured prominences and sierra. Nothing more beautiful can be conceived than the fringe of coloured light surrounding the intensely white orb of the sun. The varieties of colour mentioned above seemed now to be multiplied fifty-fold. There are no terms by which the beauty of the scene can be described. To say that the sun appeared like a shield of glowing silver set round by myriads of sparkling jewels of all the colours of the rainbow, is as far from the truth as though one should compare the hues of the most brilliant fireworks with the sombre tints of autumn foliage.

The glowing inner atmosphere amidst which these prominences displayed their splendours, had now, owing to our near approach, increased very largely in apparent extent. We could distinguish many varieties of colour and brightness within its limits, and from time to time radial striations appeared over the solar spot

zones, though they showed but faintly compared with those we had seen earlier, and remained visible but a short time. When they were most clearly seen they could be traced outwards into the less luminous atmosphere, which we could now distinguish to a vast distance from the sun's surface. This outer atmosphere was not irregular, as we might have judged from the earlier appearance of the radiations; for we could now see that those radiations had been wholly within the limits of this exceedingly rare atmosphere. We could trace the envelope to the distance of about eight millions of miles from the sun on all sides; at which distance it appeared to have a definite boundary. But outside, as well as within its limits, the irregular streams and sprays of whitish light could now be seen with greatly enhanced distinctness, and could be traced to a much greater distance from the sun. It had become perfectly obvious to us that these whitish streaks were due to myriads of meteor systems existing in the sun's neighbourhood. We had long since observed how much more richly these systems were congregated close by the sun; and the nearer we ourselves approached his orb the more surprising was the richness of meteoric aggregation. We now encountered, not systems of meteors, but systems of meteor systems; while amidst these systems, and seemingly associated with them, were countless thousands of those relatively minute comets which have been already referred to. That these comets glowed chiefly with their own inherent lustre, we could not doubt; but the meteor systems shone by

reflecting the sun's light; and we could already perceive how much more brilliantly they are illuminated than the meteors which pass close by the earth. For the sun presented a disc many times larger than as he appears to the terrestrial astronomer. So that the meteor systems, infinitely more numerous as well as severally richer in the sun's neighbourhood, and illuminated many times more brightly, formed a conspicuous but irregular halo around the sun. We could perceive also that as their motions (far more rapid than those of the meteors first encountered) carried groups and clusters of them into the solar atmosphere, they began to glow with inherent light, partly, no doubt, because of the increased heat to which they became exposed, but chiefly, as I judge, because the sun's electrical action was then more freely communicated to them. I do not suppose that atmospheric resistance can have been in question, since even such tenuous bodies as comets pass far nearer to the sun without being appreciably affected by this cause.

It was the sudden access of brilliancy in meteor systems close by us, which gave us the first intimation that we were about to cross the boundary of the solar atmosphere. We were all prepared, as we thought, to experience in some striking manner the effects produced as we passed from the ether of interplanetary space into the sun's atmosphere,—infinitely rare though it might be at this distance from his surface. But we were in no sense prepared for the surprise which actually awaited us. Of a sudden we passed from absolute

silence to an uproar infinitely surpassing the tumult of the fiercest terrestrial storms. We were still some eight millions of miles from the sun, yet the tremendous processes at work within his domain produced the most stupendous reverberations even at that enormous distance, and in an atmosphere rarer than the so-called vacuum of the experimentalist. Nothing in all our progress thus far, had given me so startling an insight into the mighty energy of the sun, as this amazing circumstance. Somehow I had always associated the idea of perfect silence with the solar activity; and perhaps it had been on this account that I had hitherto experienced a sense of unreality when considering the mighty processes at work, as telescopic research had shown, in the solar orb. But now that I could, as it were, hear the working of the mighty machine which governs our scheme of worlds,—now that I could feel the pulsations of the great heart of the planetary system,—the sense of the sun's amazing vitality was brought home to me, so far at least as so stupendous a reality can be brought home to the feeble conceptions of the human mind.

Amidst a continually increasing uproar, and through an atmosphere so intensely heated that no creature living on the earth could for an instant have endured its fiery breath, we passed onwards to the glowing inner atmosphere, and still onwards to the very limits of the sierra,—where it seemed fit that our course should be stayed in order that we might contemplate the wonders that surrounded us. It would be useless

for me to attempt to describe all that we had witnessed during this last stage of our voyage to the sun; wonders had surpassed wonders, glories that had seemed incredible had become lost in yet more amazing glories, each moment had seemed to bring the climax of splendour, of fierce energy, of inconceivable uproar, and yet at each moment we seemed as though we should forget the wonders we had witnessed in those which were being newly revealed to us.

We were now within twenty thousand miles of the sun's surface. All round us were waves of flaming hydrogen into which uprose continually vast masses of glowing vapour resplendent with all the colours of the rainbow, if a rainbow can be conceived as of intensest fire. Some thirty thousand miles from where we were, a mighty prominence towered aloft to a height of at least seventy thousand miles. We had arrived close by the spot zone, and between us and the prominence the surface of the intensely bright photosphere was tossed into what appeared as the immense waves of a white-hot sea. We could perceive that along the whole length of the prominence, even to its very summit, which seemed to be almost vertically above us, a rush of fiery vapour was passing continually upwards with incredible velocity. From time to time masses of matter which resembled molten metal were expelled as if from a vent far beneath the lowest visible part of the fiery column. After each such outburst, the prominence seemed to glow with increased brilliancy, its shape also changing, as though the surrounding atmosphere were agitated by

tremendous hurricanes. But even as we watched, the explosions grew less fierce. Presently they ceased; and then the whole prominence, vast as was its extent, seemed to dissolve, until in an incredibly short time no trace of it could be perceived.

But a circumstance that surprised us greatly was this. Although the uproar and tumult which prevailed were inconceivably great, yet, during the whole progress of the solar eruption which we had been witnessing, there were no sounds which we could associate with the tremendous outbursts which must in reality have taken place. Accustomed to associate terrestrial volcanic explosions with sounds of exceptional loudness, we were amazed to perceive no distinctive sounds during the infinitely mightier eruption we had just watched.

But as we passed towards the scene of the eruption—eager to contemplate the effects of an outburst competent to destroy the whole frame of a globe like the earth—the mystery was explained. While we were still far from the place of explosion, and intent on the study of the great facular waves which were passing swiftly beneath us, we suddenly heard a series of explosions so tremendous that we imagined a new eruption was commencing close by. Yet we could perceive no signs of unusual solar activity. All round our horizon, indeed, we could discern prominences of greater or less dimensions: but these we had observed before. Whence then came the fearful noises now reverberating through the solar atmosphere?—noises so tremendous, that the unutterable uproar which had prevailed unceasingly all

round us, seemed hushed, by comparison, into perfect stillness. X. was the first to see the meaning of the phenomenon. These sounds were those produced during the explosion which had ceased some time before; the interval which had elapsed corresponding to the vast distance which still separated us from the scene of the outburst. Just as a perceptible interval elapses between the flash of a gun and the moment when the noise of the discharge reaches the ear of a distant observer,—so in the present case a comparatively long interval elapsed before the sound-waves traversed the distance which light had traversed in less than a second.

As we approached the scene of the outburst, we perceived that we were nearing the borders of an enormous region which seemed dark by comparison with the intense brilliancy of the rest of the photosphere. The faculæ, forming here immense ridge-like waves, prevented us for a time from fully discerning the nature of this region; but after we had passed some of the loftiest of these seeming waves, we could perceive that the dark region formed a sort of lagoon, though of an extent exceeding the whole surface of the earth. We had, in fact, approached one of those regions which terrestrial observers call spots. We could readily infer that the spot was not one of the very largest; in fact, it was little more than twenty thousand miles in width. We found that (as astronomers have inferred) the dark region lay below the general level of the photosphere. But terrestrial observers have wholly

underrated the extent of the depression of these regions. The reason of this X. considered to be the refractive power of the dense atmosphere within these depressions, which causes them to appear shallower than they really are, much as a basin when filled with water appears shallower than it really is. We judged the depth of the depression in the case of this particular spot to be fully ten thousand miles.

Placed as we were now at the borders of an enormous sun-spot, we could understand the real meaning of some of those appearances which had seemed perplexing during the telescopic scrutiny of the sun. In the first place, we could perceive that, throughout the whole extent of the depression before us, there was the most intense activity; but the most violent action took place all round the borders of the spot. We could see, in fact, that several of the prominences we had observed during our progress sprang from the borders of the relatively dark depression; and though scarcely a trace remained (to our great amazement) of the mighty eruption we had so lately witnessed, we could judge from the aspect of the region we had reached, that *here* (on the nearer border of the spot) that tremendous outburst had taken place. All round the spot immense waves of *faculæ* raised their glowing crests above the general solar level; and we could see that this was due to the action of some cause by which the matter of the photosphere had been driven outwards from the region of the spot, and had so become heaped up in great ridges all round. Descending to a lower level, we found

that this photospheric matter was actually of the nature of cloud or fog, and that it was, in fact, formed by the condensation of the glowing vapours of many metallic elements into innumerable globules or vesicles, resembling the water vesicles of our clouds. From the inner surface of some of these clouds we could perceive that metallic rain was falling. The metallic showers were particularly heavy on the borders of the spot, though whether this was due to the cooling to which the region of the spot appeared to have been exposed, or to electrical action caused by the intense activity all round the spot, we could not satisfactorily determine. And though we visited several other spots—one of them remarkably large—we could perceive nothing explanatory of these localised showers.

In passing over the general photosphere—that is, over regions where there were no spots—we saw no signs of the objects which have been called willow-leaves. The photosphere presents a curdled aspect, as though the metallic clouds which produce the greater part of its light had been agitated into somewhat uniformly-disposed waves—not rollers, but such waves as are seen when two seas meet—but there was nothing suggestive of interlacing. In the neighbourhood of the great dark depressions, however, the rounded clouds seemed to be lengthened by the effects of atmospheric disturbance; an effect which was enhanced by the downfall of metallic showers from these clouds. X., who had been inclined to entertain the belief that the bright solar willow-leaves are in some sense organised beings, ad-

mitted at once that nothing in their aspect, on a nearer view, encourages such a conception of their nature.

We visited both spot zones, and examined many spot depressions in several stages of development. From what we saw, we were led to the conclusion that spots are caused, in the first instance, by the arrival of matter from without, under such circumstances as to cause a large portion of the solar atmosphere to be cooled. It was clear, indeed, that much of the matter which continued to arrive from without caused a local increase of the sun's heat. This was especially the case with matter which arrived nearly on a vertical course. But other matter, which descended less rapidly to the surface, produced a precisely contrary effect, and as it settled down in the solar atmosphere, displacing and driving outwards the intensely bright solar clouds, it appeared to cool the underlying matter in such sort as to cause it to shine less resplendently then elsewhere. But all round a region thus cooled, intense eruptive action was invariably excited, every spot we visited being literally circled about by prominences of greater or less size. Some of these eruptions were so amazingly active that the ejected matter (which seemed to come from an immeasurable depth) was propelled with a velocity even exceeding that of any of the matter which arrived from without; so that we could not but conclude that the matter thus disgorged was driven wholly and for ever away from the sun. There were signs which led us to believe that intense electrical action was excited during these eruptions, and it does not

seem unlikely that such action may afford the true explanation of the radiations seen in the outer solar envelope.

Although not liable to any sense of fatigue, and impervious to any of those risks which seemed to multiply around us, we began to be bewildered by the succession of wonders which had been revealed to us. Y., in particular, wished to escape from the fierce light and the dazzling colours, as well as from the inconceivable uproar and tumult, which we had now experienced, for some hours in reality, but for an age to our perceptions. X. was desirous of penetrating deeply beneath the photosphere, in order to obtain an answer to some of those questions which have lately arisen respecting the condition of the sun's interior. He suffered himself, however, to be overruled, though exacting from us a promise that this, our first voyage to the sun, should not be the last.

Shall I tell you the thought that chiefly occupied us as we returned to the earth? On all sides were countless myriads of stars; in front, the mighty convolutions of the galaxy, infinitely complex in star-texture; directly below, the great Magellanic cloud, full of stars and star-clusters; suns everywhere, of every order of magnitude and splendour. We had wondered at the beautiful spectacle presented by the sun of our own system; but now that we had visited that sun, and had learned something of its amazing might and activity, the thought seemed awful, nay, almost appalling, that all those suns, as well as the unnumbered millions

we could not perceive, were of like nature,—that the infinitely wonderful scene we had just beheld was thus infinitely multiplied throughout the infinite universe of the Almighty.

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A VOYAGE TO THE RINGED PLANET.

At midnight on the 9th of July 1872, Saturn being at the time due south and not far above the horizon, we set forth on our voyage across the depths of space which separate this earth from the Ringed Planet. The voyage we were now undertaking was of far greater extent than that to the sun which I have already described. Nearly nine times as far we were to travel, and that not towards the glorious centre whence light and heat are dispersed to the members of the planetary scheme, but to regions where his influence is diminished a hundredfold, where for aught that we as yet knew, an unendurable degree of cold may prevail, and where life must exist under conditions altogether different from those with which we were familiar. Yet I must confess that, deeply as I had been interested when we set forth on our journey to the sun, I was yet more interested on this occasion. Wonderful are the mysteries of the sun, stupendous his bulk and might, past conception his glory; yet the human sympathies are more directly affected by the thought of what may exist in worlds

resembling our own. The grandeur of the universe is incomprehensible, 'the glory of God is insufferable;' but in other worlds we may find creatures as imperfect as ourselves; there we may witness phenomena that we can understand because they are comparable with those already known to us—in such worlds, in fine, we may find safety from 'the persecution of the infinite.'

It was with a strange feeling that we watched the earth gradually passing from our view. It was night. Our course was directed towards the darkest region of the heavens, and as the faint lights which shone from towns and villages beneath us grew undiscernible with distance, we were immersed in a profound darkness, which seemed so much the more awful that around us was almost vacant space. As in our former journey, the sounds of earth gradually subsided into perfect stillness; though again as we passed the confines of the air what had seemed stillness appeared to us as uproar by contrast with the silence of interplanetary space. We passed rapidly onwards, directing our course almost exactly towards Saturn (now shining very conspicuously in a somewhat barren portion of the constellation Sagittarius), but giving our attention chiefly to the orb which we had so lately left, for we were curious to know how the earth would appear when viewed from its night-side. We could readily recognize the earth's shape because the stars were now shining with great splendour, in numbers enormously exceeding those which can be seen from the earth on the darkest and clearest night; and there was a vast circular disc of

darkness where stars were blotted from view by the earth's globe. We could see this dark disc gradually contracting, like the pupil of an eye, as we travelled onwards, and we could in some sort estimate our position by noting the dimensions of this gigantic eye, whose iris was the star-bespangled sky, while its pupil was the great globe itself which men inhabit.

Presently, as we travelled onwards, the moon appeared on the left of the earth. So soon as her full disc was uncovered we saw her as a fine sickle of light. But to our astonishment the rest of her disc was particoloured. The part farthest on the left was perfectly black, its outer outline only distinguishable because projected on the starlit sky. This part formed a black sickle almost exactly opposite to the sickle of true moonlight. But between the white and black sickles lay a half-lit space of a bluish-green colour. This colour was well marked, and we were at some loss to account for it, until X. pointed out that this part of the moon's surface was illuminated by earthshine chiefly coming from the Pacific Ocean, whence doubtless proceeded the beautiful tint which was spread over the middle of the lunar disc.

Passing farther away, we saw that the left side of the earth's disc began to be illumined by a faint light received from the moon. Elsewhere, however, the disc of the earth continued perfectly dark, until we began to approach the orbit of the moon, when we could perceive that all round the earth's disc a deep red light was making its appearance. Before long we

saw that this was actual sunlight. The earth's globe at this time presented a marvellous appearance. Its apparent diameter was about four times as great as the moon's (not as then appearing to us, but as she appears when seen from the earth); but all round this large dark disc we could see a ruddy light of extreme brightness, and growing gradually brighter as we receded. At length, while the earth's disc was still ten or twelve times larger than that of the sun or moon as seen from the earth, we could perceive that the red light was as bright as the rising sun. It was indeed actually the sun, rising into our view; but instead of rising opposite one part only of the earth's dark disc, the sun was rising (if I may use the expression) all round the earth; only in one or two places the bright red ring was interrupted, and opposite these regions the red glare beyond was somewhat fainter. But what seemed to us an amazing circumstance was to see the sun actually transformed into a red ring of light, having an apparent diameter more than three times greater than that which he ordinarily presents. This must appear so incredible, that I fear many may be disposed to consider that we were in some way deceived; or even, in consequence of the doubts thus suggested, to disbelieve this narrative altogether; but it is my intention to describe what we actually witnessed, without inquiring how far it may seem likely or unlikely to those whom this narrative may reach.

I would willingly enter upon X.'s ingenious explana-

tion of the spectacle now presented to us, as well as of the varying aspect presented by the sun as our distance gradually increased. But I am told that it is desirable for me to turn from the narrative of these phenomena, in order to present the record of that part of our journey which relates more particularly to the planet Saturn. Let it suffice, then, to mention that the bright ring of light which was for the nonce our sun, contracted gradually in diameter as we receded, increasing continually in brightness. Later we reached a stage on our journey when the earth began to be presented as a vast black disc upon the solar face, now no longer magnified by the effect of the earth's atmosphere. This black disc grew smaller and smaller, until presently another smaller disc—the moon's—appeared along with it on the sun's face. At this time we had passed somewhat beyond the path of Mars, and we turned from the further contemplation of the earth and moon, in order to give all our attention to the circumstances of our journey towards the ringed planet.

Saturn now appeared much brighter than we had ever before beheld him. Our course thus far had carried us almost directly towards him, though a very slight deviation northwards had to be made so soon as we ceased to direct our path by keeping the earth on the middle of the sun's face. We had had a special object in this, as X. was very desirous of studying the varying appearance of the earth as we so travelled. Now, however, we travelled directly towards the rich golden

orb of Saturn. We could not at present see the ring, nor, indeed, any sign that the planet is not like other planets. Saturn shone there before us, distinguished only from the stars by his superior brightness, and a certain indescribable contrast between his light and theirs. For though the stars were not twinkling, but shining with 'purest ray serene,' yet was there something in the stellar light which caused it to differ unmistakably from that of Saturn. It may have been partly, perhaps, that, owing to the exceeding swiftness of our onward flight, we unconsciously recognised the comparative nearness of Saturn, and were thus impressed by the distinction between the light from suns millions of times farther from us, and that from an orb which, vast though it is, is yet insignificant compared with the least of the suns which people space.

We passed through the zone of asteroids, and I could tell you much that would interest you respecting these small bodies; but it will be better to reserve such details for another occasion. Let it suffice to mention that astronomers have not yet discovered the thousandth part of this family of small planets. Even crossing the zone at one particular point we saw more asteroids than astronomers have yet counted; though certainly hundreds of those we saw were so small that astronomers could not hope to see them with the telescopes at present in use. Not even the largest that we passed presented any signs of being inhabited or fit for habitation. But the asteroids are not fragments of a

larger planet. Every one of these bodies is as well rounded an orb as the earth on which you live.

Swiftly we traversed the enormous gap separating the outermost part of the zone of asteroids from the path of Jupiter. Although this planet was on the opposite part of his orbit, we could recognise our approach to his course by a circumstance which caused us no little surprise. We found many small comets travelling slantwise across our path in this neighbourhood. Probably they belong to a system consisting of comets which have been attracted from their former course round the sun by the mighty energy residing in Jupiter's mass, and have thenceforth continued to circle in paths crossing that of the giant planet. If so, their real number must be enormous; for of course we only saw a few of those which happened at the time to be rounding the part of their path near Jupiter's orbit, or rather, near that part of Jupiter's orbit which we crossed in journeying to Saturn.

When we were within about ninety millions of miles from Saturn, we began to recognise the shape of the Saturnian rings. The planet was now a glorious object. It was shining far more brilliantly than Jupiter or Venus when at their brightest; and its rich golden yellow hue distinguished it from all that we had hitherto seen in the heavens. There was no orb within our view, save the sun alone, which could be compared with this golden oval in splendour, though the whole of the celestial sphere, spread over with a hundred thousand stars, was open to our contemplation. Behind

us lay the sun, whose disc was barely equal to the seventieth part of the orb he shows to the earth. Directly in front lay Saturn, looking nearly as large as the sun, though infinitely less brilliant. Besides these two orbs, the heavens presented only bright points of light; and the earth we had so lately left was now altogether undiscernible.

Impressed with a sense of utter loneliness,—for save where some vagrant meteor flashed past us, we saw no created thing within ninety millions of miles,—we exercised the powers we possessed to their utmost, in order to reach the planet which we recognised for the time being as a home prepared for us. Saturn grew under our view, so swift was our onward flight; his ring-system became more and more clearly discernible; and his satellites could now be clearly distinguished from the star-bespangled background upon which hitherto all but the two brightest had been lost. We had determined to pass straight to the planet's globe, a course which would carry us above the nearest part of the ring-system. I say 'above,' though in reality 'above was below, and below was above,' stripped as we were of gravitating body. We were in fact to pass athwart the northern face of the rings.

As we neared the planet, though as yet we were far beyond the path of the outermost satellite, we could perceive that the golden colour which had formed so beautiful a feature of Saturn, came from certain parts only of his globe; or rather, a much deeper tint, a burning cinnamon (so to describe at

once the intensity of the colour and its peculiar hue), came from certain zones of the planet. Even these zones seemed mottled, insomuch that we were prepared to find that on a nearer approach their tint would be found to result from a mixture of various colours. But between them were zones quite differently tinted. The actual aspect of the planet may be thus described: the great central zone, occupying the position of the planet's equator, was of a bright yellow, so flecked with spots of pure white that when we had been somewhat farther away it had appeared almost perfectly white. Then came on either side zones of a rich purple flecked with yellow spots, between which were the 'burning cinnamon' bands already mentioned. But the purple of the zones became more and more bluish the farther the zones were from the equatorial belt. Close by the north pole were several narrow zones of a delicate blue; and the pole itself was occupied by a wide region of rich cobalt blue, flecked with purple and olive-green spots. The southern polar regions were as yet concealed from our view by the rings. There was a symmetry and beauty in the whole aspect of the planet which cannot be described. The rings added largely to the effect; they also presented a singularly charming arrangement of colour. We could already perceive that the outer ring was divided into two distinct rings, and also by several circular gaps not extending completely round, while the chief ring (the second great ring inwards) appeared very singularly striped by a series of dark concentric markings. Both

these rings shone with a yellow light, the dark markings presenting a sepia tint, while the great division between the two rings, instead of being black as we expected, was of a deep brown-purple colour. Somewhat similar, but more richly purple, was the so-called dark ring, except that where it crossed the planet's disc it appeared to shine with a full brown colour. The shape of the globe, and even the figure of certain markings upon it, could be distinctly seen through the dark ring. We even thought that we could trace the shape of the globe through the inner part of the second bright ring, and subsequently we found that we had not been deceived in this respect.

In order to avoid confusion it will be well that I should omit further reference at present to what in reality occupied no small share of our attention as we approached Saturn's globe. The marvellous aspect of the rings must be described farther on. For the present I shall speak only of the globe of the planet.

To our amazement we found, as we drew nearer to Saturn, that his whole surface presented a scene of indescribable agitation. The white clouds on the equatorial belt appeared and changed in shape and vanished with startling rapidity. And the whole of this belt seemed opalescent, the colour and brightness of the different parts varying continuously. These changes had not been noticed by us when we were at a greater distance, because they did not affect the general lustre or colour of the zone, or even of large portions of its extent. But now they were perfectly distinct, and each

moment growing more marked in character. I do not know how I can better illustrate the nature of the changes taking place in the great equatorial belt, than by comparing its appearance to that of shifting clouds of steam strongly illuminated by concealed fires. The neighbouring belts were equally changeful in aspect; but they presented at all times a much greater depth and variety of colour. It was as though not white steam-clouds, but masses of coloured gas were illuminated by a continually changing glow. The colours were even more variegated near the planet's poles; though here the changes were less rapid and remarkable. The general blue colour of these regions seemed to be due to the presence of an overhanging pall of blue vapour, through which from time to time a purplish glow could be recognised in certain spots.

These appearances were so remarkable, and seemed so obviously to belong to the planet itself, and not to be caused by the varying effects of the sun's light, that we determined as we drew near the planet (and when we were already past the inner edge of the dark ring) to circle round Saturn's globe so as to reach its unillumined side, before passing beneath the planet's atmosphere.

We did so, penetrating into the vast shadow projected by the planet into space. Instead, however, of the black darkness which might have been expected, we found that all the part of the planet which at the moment was turned from the sun, was aglow with a somewhat dull luminosity, like that of fire shining through

smoke or vapour. There was no night, and seemingly no rest on the half of the hemisphere turned from the sun. Occasionally, we could even see an intense luminosity spreading over wide regions of the planet's surface, and then presently sinking into a dull glow as of heated metal. This was in the planet's equatorial regions: though at rare intervals a somewhat similar phenomenon could be recognised along other zones. The polar regions alone were dark, save where a very faint and dull luminosity became momentarily apparent. But this light was even fainter than the dull glow constantly manifest over the equatorial and neighbouring zones.

We began to perceive that whatever else of interest we might find in the globe of Saturn, we need certainly not look for living creatures there. It was plain that we were about to visit a region where nature's forces were working too intensely to admit of other and less active forms of force. We became cognizant indeed of another circumstance, which confirmed this impression. As we approached the globe of Saturn, we could perceive that myriads of meteors and small comets were circling close around him, or streaming in upon his surface. They travelled much less swiftly than those which we had seen in the sun's neighbourhood; but still their velocity was enormous, insomuch that their fall upon the planet or their swift rush through his atmosphere would have sufficed to destroy all living creatures on his globe. But the fiery glow of so large a proportion of Saturn's visible surface, seemed of itself sufficient to show that it could not be inhabited.

When at length we passed within the Saturnian atmosphere,—which extends but a small distance relatively above his visible surface,—we obtained at once the most convincing evidence that he cannot possibly be the abode of life. Immediately a strange uproar surrounded us, less intense but scarcely less appalling than that which prevails within the solar atmosphere. Repeated reverberations seemed to announce either the collision of enormous masses or the occurrence of tremendous volcanic outbursts. But the most characteristic of the noises which greeted us was an intense and persistent hissing, as though steam was rushing from a million outlets at once.

Passing to the illuminated portion of the planet—and remaining on the equatorial zone—we found our selves still unable to tell whence this hideous noise proceeded. On all sides of us were immense masses and columns of whitish vapour; some rushing violently to vast distances above us, others sinking, others quiescent in position, but rapidly changing in figure. Directing our perceptions towards the depths beneath us, we could recognise no sign of any surface. We passed downwards for hundreds and hundreds of miles, until we had lost the light of the sun, which was replaced by the continually increasing glow of the fires we were approaching. At length, as we passed through a layer of clouds, which could scarcely have been less than twenty thousand miles below what we had regarded as the surface of the planet, we suddenly beheld a scene so startling that we stayed our course as by

common consent to gaze upon it. We at length saw the true surface of Saturn. And what a surface! For land and water we saw glowing rock and molten lava. Vast seas of fire, tossed by furious gales whose breath was flame, coruscated with a thousand colours as their condition underwent continual change. Then over a wide extent of those oceans the intense lustre would die out, to be replaced by a dull almost imperceptible glow, where the surface of the fiery ocean was changing into a crust of red-hot rock. But then came fresh disturbance; the crust broke in a thousand places, showing the intensely hot sea beneath. Fragments of red-hot rock, many miles in extent, were tossed hither and thither by the raging sea. Nor were these the only evidences of an intense energy. For from time to time the rush of the hurricanes which raged over the molten oceans was hushed into comparative stillness as volcanic explosions took place, the least of which seemed competent to destroy a world. Enormous volumes of steam and of other imprisoned gases were flung upwards with irresistible force, bursting their way through the overhanging canopy of cloud, and passing to heights where from our present standpoint they were wholly lost to view.

We should have wished, perhaps, under other circumstances, to extend our survey over the rest of Saturn's surface; though from what we had already witnessed, we felt well assured that the whole planet is the scene of a turmoil and confusion resembling that now before us. At the poles indeed there is an approach to

quiescence, and it would even appear that before many ages are past, the polar Saturnian regions may be fit to be the abode of living creatures. On the other hand, the equatorial zone of Saturn seems to be in a state of abnormal activity; and though this may be in great part due to the intensity of the subterranean forces at work here, and to their partial relief from the action of gravity, yet it seems chiefly to be occasioned by the continual downpour of cometic and meteoric matter over this zone. Even during our short stay the dense atmosphere around and above us was roused more than once into tremendous whirlwinds by the arrival of enormous masses of matter from without. But though local peculiarities of this sort exist, yet, in a general sense, it may be said that the whole bulk of Saturn is instinct with fiery energy, rendering it altogether unsuited to be the above of living creatures, or at least of creatures resembling any existing on the earth. If creatures of another kind exist there, we could recognise no sign of their presence. If there are intelligent beings there, their intelligence is not such as human intelligence can communicate with. We agreed that so far as the evidence before us went—and as I have told you our powers of perception were limited—Saturn, like the sun, is altogether uninhabited. It is the scene of an intense physical activity, but no form of vital energy exists there, nor are any of the processes at work there due to the action of any form of intelligence.

We passed from the burning surface of Saturn,

through his intensly heated and most perturbed atmosphere, sharing a common sense of regret that our journey had as we thought been fruitless. We had indeed seen much that was wonderful, and much that we had hitherto had no conception of; but we had set out on our voyage with the hope of discovering other living creatures in Saturn, and we had found not only that none such exist, but that this giant orb is altogether unfit to be the abode of life. We agreed, however, to carry out our original plan,—to visit the rings and satellites before returning to the earth. The rings naturally received our attention in the first instance.

So soon as we had passed beyond the atmosphere of Saturn, we found that during our stay the planet's swift rotation had carried the region we had been visiting to a considerable distance from the place it had before occupied. We had indeed been only some three hours under the veil of clouds forming the surface of the planet as he appears to terrestrial astronomers. But three hours on Saturn, *at his real surface*, correspond to nearly half a day on the earth, though observers on the earth (mistaking the cloud-regions for his surface) call his rotation-period ten hours. Accordingly, we were at a loss at first to know precisely where we were. And I would note in passing that none should undertake such voyages as ours without a considerable share of astronomical knowledge, lest haply having lost sight of the world they had left, they should be unable to rediscover it. Where we were, there was indeed little risk of this, as the rings and satellites

indicated sufficiently the position we were in. We had but to look towards the heavens to see the tiny but brilliant orb which is the sun of Saturn, and at once we knew where Saturn's rotation had carried us.

We now passed to the so-called dark ring. This ring is, however, no darker, in one sense, than the others. The seeming darkness and brightness of the rings are not at all due to the darkness or brightness of the matter composing them. The fact really is, that the dark ring consists of a number of very small bodies, all travelling nearly in the same level, and so widely scattered that the observer on earth can see through the ring the deep blue background of the sky. This deep blue background, combined with the yellowish red light which these bodies reflect, produces the purplish brown colour which terrestrial telescopists recognise in this ring.

But when we reached the ring we found that the small satellites are immersed in a vaporous envelope, not forming atmospheres for the satellites severally, but constituting a somewhat flattened ring of vapour, through which they travel. They actually carry with them, however, considerable masses of this vapour; and hence some very remarkable effects follow. For though the satellites are severally minute, their vapour-coats extend pretty widely, and thus, though collisions rarely occur in this ring between the actual satellites, their vaporous envelopes are continually encountering, so that the general atmospheric ring is loaded with detached vaporous masses which only diffuse themselves

very gradually into the surrounding and much rarer atmosphere.

When we actually entered this atmosphere, we found that a noise as of a mighty whirlwind continually prevails within it, while, from time to time, thunderous reverberations are heard which echo and re-echo as though they would never cease. We were at some loss to conceive the cause of this tumult, since we could perceive that collisions between satellites were few and far between. Nor, indeed, were such collisions of a nature to cause any such uproar as occasionally arose, for it was worthy of notice that all the satellites were travelling the same way round, though not in perfectly circular paths,—so that there were no direct encounters. All that happened was that, from time to time, a tiny satellite would overtake another and come into contact with it: and even such collisions seemed to be softened by the atmospheric surroundings of these bodies.

But as we traversed the width of the dark ring and approached the main edge of the great bright ring, we perceived that one atmosphere envelopes the whole of the ring system, insomuch that collisions taking place in one part of the system are audible in other parts. Now the bright rings consist, like the dark ring, of millions of minute satellites, but these are spread much more densely. Rising for a few moments out of the atmosphere of the rings, we could perceive that the dark background of sky was readily discernible through even the brightest part of the ring; and passing down again through the atmosphere and so beyond to the

other side of the ring, on which the sun was not shining, we found that not only could the dark background of sky be perceived, but that it was possible to recognise the constellations through the Saturnian rings! But although it may be conceived from this circumstance that the satellites composing even the brightest parts of the rings are not very closely set, yet collisions are very numerous in the brighter zones of the rings. When we were passing through one of these zones the reverberations were almost continuous, and were at times so tremendous that we could readily understand their being audible even in the dark ring, ten thousand miles away.

In passing, I must not omit to notice a circumstance which struck us as interesting. When we passed through the rings to a great height above their level, we could readily trace the motions of the satellites composing the rings. But as we approached the level of the rings again, the rapidity of these motions prevented us from discerning the separate satellites, unless we chose to follow their course. When we remained still, they flashed past in such sort as to cause the ring to assume the appearance of a network of bright streaks, of greater or less length according to the greater or less rapidity with which the particular satellite producing any streak was moving. The continual change of appearance of this network as the several streaks shifted, was one of the most beautiful sights I ever remember to have witnessed. It reminded us in some degree of the appearance presented when a calm

sea is traversed by a series of cross-ripples, whose sun-illumined crests form a shifting network of light.

After traversing the width of the inner bright ring, we reached the so-called gap between the rings. But this is no real division. It is very similar indeed to the dark ring, and only appears darker than the neighbouring rings because it is occupied by few satellites, whereas they are formed of many. It is remarkable, however, that during the time of our stay in this part of the ring-system we did not perceive a single satellite within it whose course was parallel to the sides (or, if one may so speak, the shores) of the dark ring; every satellite we saw passed from the inner bright ring outwards, or from the outer bright ring inwards; and moreover, every such satellite returned to the ring from which it had come—not one passing athwart the whole breadth of the dark region.

We passed through the outer bright ring, noticing nothing that in any remarkable degree distinguished it from the inner bright ring. In both these rings the satellites showed a tendency to travel in long flights, so as to form as it were subordinate rings, or rather parts of rings, for these flights nowhere extended more than a few thousand miles in length.

All the most interesting part of our voyage was now, as we supposed, past. We had only to pay a hasty visit to each of Saturn's eight satellites, and then to return, heartily disappointed, so far as our main object was concerned, to the world we had left in such high hope.

As Mimas, the innermost satellite, was close by the

part of the ring-system we had now reached, we passed over at once to this small orb.

Prepared to find in Mimas a miniature moon, even less interesting than it might otherwise have been, because we knew now that it could serve no useful part to living creatures in Saturn, our amazement will be conceived when we discovered as we approached that Mimas is a miniature world. We saw before us land and water; we could perceive clouds floating in the Mimasian air; and presently as we passed the confines of this air, we began to hear the sounds of busy life. Descending through a cloud veil which hid from our view the land and water immediately beneath us, we saw at length the beings of another world!

At first all was perplexing to us. We perceived living creatures utterly unlike any with which we had hitherto been familiar. They were busy in their several ways, but the nature of their ways and the object of their actions we could not comprehend. It would only confuse those whom this narrative will reach to describe all that we saw, or to attempt to explain how what we saw became gradually intelligible to us. The forms of life are probably almost as numerous in Mimas as on the earth; and the relations between the several orders of living creatures are as interesting and as complicated. It would require a whole treatise to present aright all that a Huxley or an Owen in Mimas could teach about the living creatures which exist there. It is clear that to convey accurate ideas respecting the whole economy of another world would be quite im-

possible, unless those to whom I address this narrative were prepared to study a whole volume on such matters.

But certain circumstances may be related, as likely to prove interesting to the inhabitants of another world.

The Mimasians are somewhat smaller than men, but like men, they carry the head erect, and have four chief limbs, two upper and two lower, the latter chiefly used in progression. The trunk is shorter in proportion to the total height, and the frame appears to be more muscular and powerful. It is difficult, however, to form a judgment on this point, because the circumstances under which these beings live are altogether unlike those which prevail on the earth. Indeed, so soon as we had learned that Mimas is inhabited, we expected to find the creatures living here either gigantic in stature or else of surpassing agility, simply because we knew that Mimasian gravitation must be very much less energetic than the attraction of gravity on the earth. But we found none of them to exceed in dimensions the creatures most nearly corresponding to them on the earth; while there is nothing very remarkable about the activity of any Mimasian animals. It would seem likely that the question of actual strength and activity depends quite as much on other circumstances as on those which have usually been considered by writers on the subject of other worlds. We thought, for instance, we could recognise in the slowness of respiration among the Mimasians, in the small quantity of air drawn in at each respiration, and in the relative rarity of their air, sufficient reasons for

the small degree of activity which they displayed under conditions which would enable men to spring with ease to thrice their own height.

But it was in the configuration of the head that these beings were most markedly distinguished from the human race. The ears are large and quite round, somewhat resembling .conch-shells, and capable of changing in shape so as to gather in a greater or smaller quantity of sound as the Mimasian may desire. But the most remarkable feature of the Mimasian face consists of two orbits immediately above the large eye-orbits, and occupied by a series of delicate thread-like appendages radially arranged. For a long time we were quite unable to understand what this feature might signify, especially as the Mimasian animals exhibit a like peculiarity, though with characteristic differences of structure. We found at length, however, that the feature represents a sixth sense possessed by the Mimasians, and bearing the same relation to heat which eyesight bears to light. By means of this peculiar sense the Mimasian can as readily distinguish the shape of objects which approach him, as a man can tell the shape of an object lying within the range of his vision. But the sense enables the Mimasian to ascertain more than the mere shape of objects, for while his eyesight enables him to distinguish the appearance of objects, this sixth sense tells him of their constitution and physical condition. It is also as available in the darkest Mimasian night as in full day.

The axis of Mimas being inclined as well to the

level in which Saturn travels as to the plane of the ring-system (in which plane, as you are aware, Mimas circles), they have two chief seasonal influences. During the long Mimasian year (the same, of course, as the Saturnian) the sun's midday altitude changes much as on the earth; but the four quarters of the year are each rather more than seven of our years in length. These changes do not greatly affect the Mimasians, though they commonly live some ten or twelve years, that is from about 300 to about 350 of our years. (X. supposes their remarkable longevity to be due to the slowness and limited extent of their respiration.) Their chief season-ruler is Saturn himself, who supplies them with an enormous amount of heat. Indeed, the heat supplied by Saturn is so great that (as we afterwards learned) the inhabitants of Tethys, Dione, and Rhea hold life to be impossible not only in Mimas but in Enceladus, the next in order of distance from Saturn. It will be understood how important a part the heat of Saturn plays in the economy of Mimas, when I mention that he looks about nine hundred times as large as the sun appears to us. He does not indeed shine very conspicuously, the light he gives being such as I have already described in speaking of our approach to his globe. But the Mimasians have to shade their heat-eyes (so to name the feature already mentioned) when the vast orb of Saturn is in the fulness of his meridian heat-glow. Particularly is this the case when he is high above the horizon at this heat-noon. For, owing to the inclination of the axis of Mimas to the plane in which

this world travels round Saturn, the orb of the latter has a variable course on the Mimasian sky. Most perplexing are the relations thus presented. For Mimas turns once on its axis in about six hours, and travels once round Saturn in something short of twenty-three hours; so that even while Saturn is passing across the Mimasian sky, he can be seen to traverse a large space among the stars. X., who, as you know, is well versed in terrestrial astronomy, expressed the opinion that Mimasian astronomy must be difficult to master.

However, the Mimasians, though good observers (their instruments I shall describe on another occasion), have as yet very imperfect ideas respecting astronomical subjects. They suppose Mimas to be the centre of the universe; and though some of the more travelled Mimasians maintain that Mimas is either a globe or a cylinder in shape, yet the majority conceive that its surface is quite flat.

The ring of Saturn presents a very remarkable appearance in the Mimasian sky. It extends over an enormous arc, insomuch that in certain Mimasian latitudes when one end (or what looks like one end) of the ring-system is on the horizon, the other is overhead. The satellites composing the ring are not discernible from Mimas; and as the ring where it crosses the globe of Saturn cuts off a portion of his heat,—which they recognise with their heat-eyes just as accurately as we should recognise the eclipse of a portion of the sun,—they call the ring the 'cool zone.' Some of them very positively maintained, until of late,

that the ring is a phenomenon of the Mimasian atmosphere! These ill-advised astronomers have been shown to be mistaken, however; and it is now admitted by all that the ring is an appendage of Saturn.

I must leave to another occasion a fuller description of what we saw and learned in Mimas. It will be as well also that for the present I should say nothing respecting the creatures which inhabit Enceladus, Tethys, Dione, Rhea, Titan, and Japetus, for already this account has extended to a greater length than I had intended. Let it be sufficient for the present to remark that all these satellites are inhabited, and that the peculiarities which distinguish their inhabitants from each other and from those of Mimas, are as remarkable as those which distinguish Mimasian creatures from the inhabitants of the earth.

Hyperion, which terrestrial astronomers regard as a satellite travelling between the orbits of Titan and Japetus,—the giants of Saturn's satellite family,—is not an inhabited world. It is, indeed, but the largest of a ring of satellites travelling between Titan and Japetus, and bearing somewhat the same relation to the remaining seven satellites that the ring of asteroids bears to the primary planets of the solar system.

It will interest you also to learn that both Titan and Japetus are attended by small moons,—Titan by three, Japetus by five. These orbs, though exceedingly small by comparison with even the least of the Saturnian satellites, yet reflect a considerable amount of light to their respective primaries; for they travel on

orbits of very limited extent, and thus appear large. The nearest of Titan's moons, for instance, appears about seven times as large as our moon; yet it is not more than 230 miles in diameter. The inhabitants of Titan are persuaded that their moons are the abode of living creatures, but this is not the case.

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A GIANT PLANET.

DURING the month of May 1872 an evening star, which some might mistake for the planet of love, adorned the western skies for several hours after sunset. This orb was not, however, the true Hesperus, nor does it ever shine with equal lustre. It was the noble planet Jupiter, the giant of the solar system, itself the centre of a system of orbs whose movements, under the mighty influence of their ruling centre, resemble in regularity the motions of the planets round the sun. I propose to give a brief sketch of what is known respecting this planet, the noblest member of the solar system,—so far as bulk and mass are concerned,—excepting always, of course, the great central orb which rules the whole system. Much has been discovered during the last few years,—nay, even during the last few months,—to render such a sketch interesting.

We must, in the first place, dispossess ourselves of the notion not uncommonly entertained, that Jupiter

is one of a family of orbs, nearly equal in dignity and importance, and comprising the Earth and Venus, Mars and Mercury, among its members. This idea still prevails, because in our books on astronomy we commonly see a set of concentric circles, at regularly increasing distances, assigned as the paths of the several planets of the solar system. And besides, there yet remains in the modern teaching of astronomy a perceptible trace of the ancient astronomical systems, in which Saturn and Jupiter, Mars, Venus, and Mercury, played parts of equal importance.

Let it be carefully remembered, then, that the four planets which circle nearest to the sun,—the family of which our earth is a member,—differ in all their characteristics from the outer family (also consisting of four planets) to which Jupiter belongs. The whole of the inner family—the whole of the space within which its members travel—could be placed between the paths of Jupiter and his next neighbour Saturn, with a clear space many millions of miles wide on either side. The actual area between the paths of Jupiter and Saturn exceeds nearly thirty times the whole area within which the four lesser planets pursue their paths. And when we consider the dimensions of the four inner planets we find a like disproportion. Four circles representing these orbs can be enclosed within a circle representing Uranus, the smallest of the four outer planets; yet even this circumstance does not adequately represent the enormous disparity between the two families of planets: for, in fact, the

volume of Uranus exceeds the combined volume of all the inner planets upwards of thirty times. I might adduce many other illustrations of the complete dissimilarity between the inner and outer families of planets; but what has been already stated will suffice for our present purpose. It will be evident that in considering the members of one or other family, we must be prepared to meet with relations which differ not merely in degree, but in kind. We may thus, at the outset, dismiss from our thoughts the idea that the planet Jupiter is necessarily to be regarded as an inhabited world merely because the only planet we are actually acquainted with is inhabited. The latter circumstance may be an excellent reason for regarding Mars or Venus as the abode of life; but the analogy can no more be extended to Jupiter than to the fixed stars, which certainly are not inhabited worlds. We must, in fact, consider the physical habitudes of Jupiter independently of all conceptions based upon terrestrial analogies. Studied thus, he will be found, as we conceive, to hold a position in the scheme of creation differing considerably from that which has been assigned to him, until of late, in treatises on astronomy.

It is necessary briefly to state the dimensions, mass, and general characteristics of the planet, before proceeding to discuss its probable physical condition.

Jupiter has a diameter exceeding the earth's rather more than ten times, and a volume exceeding hers 1,230 times. It is not far from the truth to say that Jupiter's dimensions exceed the earth's in very nearly

the same degree that those of the sun exceed Jupiter's. But his mass, though gigantic compared with the earth's, does not altogether correspond to his bulk, for it exceeds the mass of the earth only three hundred times. So that, if the disc our astronomers see and measure, actually represents the true globe of the planet, his substance must be, on the average, much less dense than that of the earth. In fact, while the earth's density is nearly six times as great as that of water, the density of Jupiter (*thus judged*) would exceed that of water by barely one third. His vast globe rotates in less than ten hours on an axis nearly upright or square to the level in which the planet travels. This rapidity of rotation,—so great that points on the planet's equator travel twenty-seven times as fast as points on the terrestrial equator,—results in a considerable flattening of the planet's globe; insomuch that the polar diameter is less than the equatorial by about a twelfth part, or by fully 7,000 miles. And it may be remarked in passing, that this circumstance,—the fact, namely, that the poles of the planet are drawn in, as it were, 3,500 miles as compared with the equatorial regions, or 1,750 miles as compared with the mid-latitudes in either hemisphere,—affords a striking illustration of the enormous amount of energy really represented by the rotation of Jupiter. It may also be added that the velocity with which points on Jupiter's equatorial zone are carried round, exceeds the corresponding velocity in the case of all the planets in the solar system, and is nearly six times greater than

the equatorial velocity of the sun himself. It amounts, in fact, to about 7½ miles per second!

We do not propose to consider here at any length the system of satellites over which Jupiter bears sway; but this preliminary sketch would be incomplete without a few words on the subject. It is worthy of notice that although our earth in some sort resembles the outer planets in being accompanied by a satellite, yet the relation which our moon bears to the earth is altogether different from that which the satellites of the outer planets bear to their respective primaries. Our moon is by no means a minute body by comparison with the earth, and compared with Mars or Mercury she may be regarded as having very respectable dimensions. We may, indeed, look upon the moon as a fifth member of the inner family of planets,—a member inferior to the rest, doubtless, but still not so far inferior to Mercury as Mercury is inferior to the earth. In the case of the outer planets, however, and especially in Jupiter's case, moons hold an utterly subordinate position. Taking the accepted measurements, we find the largest of Jupiter's moons less than the 16,000th part of its primary as respects bulk, while its mass or weight is less than the 11,000th part of Jupiter's.*

* It is not uncommonly stated in our text-books of astronomy, that the density of Jupiter's moons is far less than Jupiter's density; and Lardner goes so far as to say that 'the density of the matter composing these satellites is much smaller than that of any other body of the system whose density is known.' But this is a mistake. All the satellites, save one, are of greater density than Jupiter, and that one—the innermost—is denser than Saturn, Uranus, or Neptune.

So that these orbs may fairly be regarded as bearing the same relation to their primary that Jupiter himself bears to his primary,—the sun. It will be seen presently that this consideration is an important one.

But the great interest of the study of Jupiter resides in the fact that being the nearest of the outer family of planets, the aspect of his globe supplies the best available means for determining the condition of the giant orbs constituting that family.

The first feature which strikes us in the telescopic aspect of the planet is the presence of a series of belts, lying parallel to the planet's equator. Usually the equatorial regions are occupied by a broad bright belt, of a creamy white colour, and bordered on the north and south by copper-coloured belts. Beyond these, again, lie alternate bright and dark belts, the dark belts growing more and more bluish in hue as the pole is approached,—while the poles themselves are usually of a somewhat decided blue colour in telescopes adapted to display such features to advantage. There are commonly two or three dark belts on each hemisphere.

Now, before inquiring into the peculiarities presented by these belts, and into the remarkable changes which have been noted lately in their general aspect, it may be well for us to consider briefly what such belts seem to imply. That they are due to peculiarities in the planet's atmosphere is admitted on all hands. And it has been usual to compare them with the trade-wind zones and the great equatorial calm zone on our earth.

The bright belts according to this view, are regarded as zones where for the time clouds are prevalent, the dark belts being regions where the comparatively dark hues of the planet's surface are brought into view. And then it has been deemed sufficient to point out that the parallelism of the zones is due to the extreme rapidity of the planet's rotation.

But setting aside the fact that the trade-wind zones and the great equatorial calm zone on our earth are, in reality, little better than meteorological myths, it must be regarded as a remarkable fact that, in the case of a planet so far away from the sun as Jupiter is, there should be a supply of clouds so abundant as to form belts discernible from the earth. Jupiter is rather more than five times farther from the sun than the earth is, and receives from him about one twenty-seventh part of the light and heat which falls upon the earth (equal surface for equal surface). Making every allowance for the possibility pointed out by Professor Tyndall, that some quality in Jupiter's atmosphere may prevent the solar heat from escaping, and so cause the climate of the planet to be not very different from the earth's, yet the direct heat falling on the planet's oceans cannot be increased in this way—nay, it must be rather diminished. It chances, indeed, that the very quality by which the earth's atmosphere retains the solar heat is unquestionably possessed by Jupiter's atmosphere. When our air is full of aqueous vapour (invisible to the eye) the escape of heat is prevented, as Tyndall has shown, and thus the nights

are warmer than where the air is dry. Now in Jupiter's atmosphere there is much water, for observers armed with that wonderful instrument, the spectroscope, have recognised the very same dark bands upon the spectrum of the planet which appear in the solar spectrum when the sun is low down and therefore shining through the lower and denser atmospheric strata. The spectroscopist knows that these bands are due to the aqueous vapour in the air, because Janssen saw the very same bands when he examined the spectrum of a powerful light shining through tubes filled with steam. So that there is the vapour of water—and that, too, in enormous quantities—in the atmosphere of Jupiter. But though we thus recognise the very quality necessary for an atmosphere which is to retain the solar heat, our difficulty is not a whit lessened; for it is as difficult to understand how the invisible aqueous vapour finds its way thus into the planet's atmosphere, as to understand how the great cloud-masses are formed.

Aqueous vapour in the atmosphere, whether its presence is rendered sensible to the sight or not, implies the action of heat. Other things being equal, the greater the heat the greater the quantity of watery vapour in the air. In the summer, for instance—though many imagine the contrary—there is much more of such vapour in the air than there is in winter, the greater heat of the air enabling it to keep a greater quantity of the vapour in the invisible form. In winter, clouds are more common, and the air seems moister; yet, in reality, the quantity of aqueous vapour is

reduced. Now it cannot but be regarded as a remarkable circumstance that, though the sun supplies Jupiter with only one twenty-seventh part of the heat which we receive, there should yet be raised from the oceans of Jupiter such masses of clouds as to form veritable zones; and that, moreover, *above* these clouds there should be so large a quantity of invisible aqueous vapour that the spectroscopist can recognise the bands of this vapour in the planet's spectrum.

Even more perplexing is the circumstance that the cloud-masses should form themselves into zones. We cannot get rid of this difficulty by a mere reference to the planet's rapid rotation, unless we are prepared to show how this rotation is to act in forcing the cloud-masses to become true belts. The whole substance of Jupiter and his whole atmosphere must take part in his rotation, and to suppose that aqueous vapour raised from his oceans would be left behind in the upper air like the steam from a railway-engine, is to make a mistake resembling that which caused Tycho Brahe to deny the rotation of the earth, because bodies projected into the air are not left behind by the rotating earth. Nor is it conceivable that belts which vary remarkably, from time to time, in position and extent, should be formed by sun-raised clouds in the Jovian atmosphere, if the planet's surface is divided into permanent lands and seas.

But we are thus led to consider a circumstance which, as it appears to us, disposes finally of the idea that in the cloud-rings of Jupiter we have to deal

with phenomena resembling those presented by our own earth.

We are too apt in studying the celestial objects to forget that where all seems at nearly perfect rest, there may be processes of the utmost activity,—nay, rather of the utmost violence,—taking place as it were under our very eyes, and yet not perceptible save to the eye of reason. Looking at Jupiter, under his ordinary aspect, even in the finest telescope, one would feel certain that a general calm prevailed over his mighty globe. The steadfast equatorial ring, and the straight and sharply defined bands over either hemisphere, suggest certainly no idea of violent action. And when some feature in a belt is seen to change slowly in figure,—or rather, when at the end of a certain time it is found to have so changed, for no eye can follow such changes as they proceed,—we are not prepared to recognise in the process the evidence of disturbances compared with which the fiercest hurricanes that have ever raged on earth are as mere summer zephyrs.

Indeed the planet Jupiter has been selected even by astronomers of repute as an abode of pleasantness, a sort of paradise among the planet-worlds. There exists, we are told, in that distant world, a perennial spring,—'A striking display of the beneficence of the Creator,' says Admiral Smith; 'for the Jovian year contains twelve mundane years; and if there were a proportionate length of winter, that cold season would be three of the earthly years in length and tend to the destruction of vegetable life.' (Of the terrestrial sort)

Even those who have denied that Jupiter can be the abode of life, and have formed altogether unfavourable ideas of his condition, have pictured him nevertheless as the scene of a continual calm, though the calm is, according to their view, the calm of gloom and desolation. They recognise in Jupiter an eternal winter rather than a perpetual spring. Whewell, for example, in that once famous work the Plurality of Worlds, maintained that if living creatures exist at all in Jupiter, they must be wretched gelatinous monsters, languidly floating about in icy seas. According to him Jupiter is but a great globe of ice and water with perhaps a cindery nucleus—a glacial planet, with no more vitality in it than an iceberg.

But when we begin to examine the records of observers, and to consider them with due reference to the vast proportions of the planet, we recognise the fact that whatever may be Jupiter's unfitness to be the abode of life, it is not of an excess of stillness that his inhabitants (if he have any) can justly make complaint. Setting aside the enormous activity of which the mere existence of the belts affords evidence, and even regarding such phenomena as the formation or the disappearance of a new belt in two or three hours as merely indicative of heavy rainfalls or of the condensation of large masses of invisible aqueous vapour into clouds,—there have been signs on more occasions than one, of Jovian hurricanes blowing persistently for several weeks together at a rate compared with which the velocity of our fiercest tornadoes seems utterly

insignificant. During the year 1860, a rift in one of the Jovian cloud-belts behaved in such a way as to demonstrate the startling fact that a hurricane was raging over an extent of Jovian territory equalling the whole surface of our earth, at a rate of fully 150 miles per hour. It is not too much to say that a hurricane of like velocity on our earth would destroy every building in the territory over which it raged, would uproot the mightiest forest trees, and would cause in fact universal desolation. At sea no ship that man ever made could withstand the fury of such a storm for a single minute. And yet this tremendous Jovian hurricane continued to rage with unabated fury for at least six weeks, or for fully one hundred Jovian days.

But during the last two or three years a change of so remarkable a nature has passed over Jupiter as to imply the existence of forces even more energetic than those at work in producing atmospheric changes.

In the autumn of 1870, my friend Mr. Browning (the eminent optician) called the attention of astronomers to the fact that the great equatorial zone, usually, as we have said, of a creamy white colour, had assumed a decidedly orange tint. At the same time it had become much less uniform in outline, and sundry peculiarities in its appearance could be recognised, which have been severally compared to portholes, pipe-bowls and stems, oval mouldings, and other objects of an uncelestial nature. Without entering into descriptions which could only be rendered intelligible by

means of a series of elaborate illustrations, let it suffice to say that the bright edges of the belts bordering on this ruddy equatorial zone seemed to be frayed and torn like the edges of storm clouds, and that the knots and projections thus formed often extended so far upon the great orange zone, from both sides, as almost to break it up into separate parts.

Now without inquiring into the particular form of action to which these remarkable changes were due, we can see at once that they implied processes of extreme energy. For, every one of the projections and knots, the seeming frayed edges of narrow cloud-streaks, had, in reality, an extent exceeding the largest of our terrestrial countries. Yet their aspect, and indeed the whole aspect of the ruddy belt, whose extent far exceeded the whole surface of our earth, changed obviously from night to night.

Strangely enough, these interesting observations, though they were presently confirmed by several well-known students of the heavens, did not attract that full attention from the senior astronomers of the day which they appear to merit. Several, indeed, of our leading astronomers were disposed to deny that anything unusual was in progress, though none asserted definitely that they based this opinion on a careful re-examination of the planet's face. But quite recently one of the most eminent of our modern observers—Mr. Lassell, lately President of the Royal Astronomical Society—(having been led to observe the planet by the fact that certain phenomena of interest in connection with the

satellite system were in progress), found his attention attracted by the marvellous beauty of the colours presented by Jupiter's belts. After describing the appearances he had intended to observe in the first instance, he proceeds, 'But this was not the phenomenon which struck me most in this rare and exquisite view of Jupiter. I must acknowledge that I have hitherto been inclined to think that there might be some exaggeration in the coloured views I have lately seen of the planet; but this property of the disc, in the view I am describing, was so unmistakable that my scepticism is at last beginning to yield.' Nor will this statement be thought to express more than the truth, when we add, that in the picture accompanying his paper, Mr. Lassell presented the equatorial zone as brown-orange, and three neighbouring dark zones as purple; one of the intermediate light belts being pictured as of a light olive-green.

Let us compare these observations made in our brumous latitudes, with those effected by Father Secchi with the fine equatorial of the Roman Observatory. 'During the fine evenings of this month,' he wrote in February 1872, 'Jupiter has presented a wonderful aspect. The equatorial band, of a very pronounced rose colour, was strewn with a large number of yellowish clouds. Above and below this band there were many very fine zones, with others strongly marked and narrow, which resembled stretched threads. The blue and yellow colours formed a remarkable contrast with the red zone, a contrast doubtless increased by a little illusion. The

surface of the planet is actually so different from that which I have formerly seen, that there is room for the study of the planet's meteorology.'

It appears to us that when these remarkable changes are considered in combination with the circumstance that on *à priori* grounds we should expect the sun to have very little influence on the condition of the planet's atmosphere, the idea cannot but be suggested that the chief source of all this energy resides in the planet itself. The idea may seem startling at a first view, but when once entertained, many arguments will be found to present themselves in its favour.

For instance, it does not seem to have been noticed, heretofore, as a very remarkable circumstance if the Jovian belts are sun-raised, that they pass round to the nocturnal half of Jupiter and reappear again, with the same general features as before, and this often for weeks at a stretch. Even that remarkable feature whose changes led to the conclusion that mighty hurricanes were in progress, changed continuously and regularly during the Jovian nights as well as during the Jovian days, for one hundred such days in succession. This is perfectly intelligible if the seat of disturbance is in the planet itself, but it is perfectly inexplicable (as it seems to me) if the sun occasions all these meteorological changes in Jupiter, as he occasions all the changes which take place in our earth's atmosphere. The alternation of day and night, which is one of the most potent of all the circumstances affecting the earth's meteorological condition, appears to have no

effect whatever on the condition of Jupiter's atmosphere!

Now, as respects the alternation of summer and winter, we can form no satisfactory opinion in Jupiter's case, because he has no seasons worth mentioning. For instance, in latitudes on Jupiter corresponding to our own, the difference between extreme winter and extreme summer corresponds to the difference between the warmth on March 12 and March 28, or between the warmth on September 15 and on September 31. Yet we are not without evidence as to seasonal meteorological effects in the case of the sun's outer family of planets. Saturn, a belted planet like Jupiter, and in all other respects resembling him so far as telescopic study can be trusted, has seasons even more markedly contrasted than those on our own earth. We see now one pole now another bowed towards us, and his equatorial zone is curved now downwards now upwards, so as to form two half ovals (at these opposite seasons), which, taken together, would make an ellipse about half as broad as it is long. As no less than fourteen years and a half separate the Saturnian summer and winter, we might fairly expect that the sun's action would have time to exert itself. In particular, we might fairly expect the great equatorial zone to be displaced; for our terrestrial zone of calms or 'doldrums' travels north and south of the equator as the sun shifts northwards and southwards of the celestial equator, accomplishing in this way a range of no less than 3,000 miles. But the Saturnian equatorial zone

is not displaced at all during the long Saturnian year. It remains always persistently equatorial! Nothing could be more easy than the detection of its change of place if it followed the sun; yet no observer has ever suspected the slightest degree of systematic change corresponding with the changes of the Saturnian seasons. Or rather, it is absolutely certain that no such change takes place.

It appears, then, that night and day, and summer and winter, are alike without influence on the Jovian and Saturnian cloud zones. Can it reasonably be questioned that, this being the case, we must look for the origin of the cloud zones in these planets themselves, and not in the solar orb, whose action must needs be largely influenced by the alternation of night and day and of the seasons?

But further, we find that a circumstance which had seemed perplexing when we compared the Jovian belts with terrestrial trade-wind zones, finds an explanation at once when we regard the belts as due to some form of action exerted by the planet itself. For let us suppose that streams of vapour are poured upwards to vast heights and with great velocity from the true surface of the planet. Then such streams starting from the surface with the rotational movement there prevailing, would be carried to regions where (owing to increase of distance from the centre) the movement due to the planet's rotation would be greater. They would thus be caught by the more swiftly-moving upper air and carried forwards, the *modus operandi* being the reverse

of that observed when an engine leaves a trail of condensed steam behind it; or rather, it may be compared to what would take place if a steam-engine were moving in the same direction as the wind but less swiftly, so that steam-clouds would be carried in front instead of behind.

Now, heat is the only form of force which could account for the formation of the enormous masses of cloud suspended in the atmosphere of Jupiter. And it seems difficult to conceive that the clouds could be maintained at a great height above the real surface of the planet unless that surface were intensely hot, as hot perhaps as red-hot iron. If we supposed this to be the case we should find at once an explanation of the ruddy aspect of the dark belts. Nor would the change of the great equatorial belt from white to red imply more than that, owing to some unknown cause, clouds had not formed during the last two years over the planet's equatorial zone, or, having formed, had been dispersed in some way. We need not even imagine a complete dispersion, since the best telescopes, and notably Mr. Buckingham's fine 21-inch refractor, have shown always a multitude of minute cloud-like objects over the ruddy equatorial zone.

But the idea of a red-hot planet, or of a planet partially red-hot, will appear at a first view too *bizarre* to be entertained even for a moment. We have been so accustomed to regard Jupiter and Saturn as other worlds, that the mind is disposed to reject the conception that they can be so intensely heated as to be utterly unfit to be the abode of living creatures.

This unwillingness to accept startling ideas is not to be altogether reprehended, since it prevents the mind from forming rash and baseless speculations. Yet we must not suffer this mental habitude, excellent though it may be in its proper place, to interfere with the admission of conclusions which seem based on trustworthy evidence. Let us then inquire whether the startling hypothesis to which we have been led by the study of observed facts may not be found to be in agreement with other facts not yet considered.

It will be obvious that if the real globe of Jupiter is thus intensely heated, a portion of the planet's light must be inherent. Therefore we might expect that the planet would shine somewhat more brightly than a globe of equal size and similarly placed, shining merely by reflecting the sun's light. Now two series of good observations have been made upon the luminosity of Jupiter. One was made by the late Professor Bond, of America, the other by Dr. Zöllner, of Germany. According to the former, Jupiter shines more brightly than he would if he reflected the whole of the light falling upon him! According to the latter, and more trustworthy series, Jupiter does not indeed shine quite so brightly as Professor Bond supposed, but the planet yet shines *three times* as brightly as a globe of equal size would shine, if similarly placed, but constituted like Mars, and *four times* as brightly as such a globe would shine if constituted like our moon. Jupiter shines in fact very nearly as brightly as though he were constituted like one of our terrestrial clouds!

This result is highly significant. If Jupiter showed no belts and shone with a pure white colour, we could explain it at once by simply regarding Jupiter as wholly cloud-covered or snow-covered (for snow and cloud shine with nearly equal lustre when similarly illuminated). But the great dark belts which occupy so large a proportion of the planet's disc altogether negative this supposition. We seem compelled to believe that some considerable portion of the planet's lustre is inherent.

Let us, however, proceed carefully here. We have to inquire first how far Zöllner's results can be trusted, and secondly, whether they are corroborated by any independent evidence. Now Zöllner carefully estimated the weight of his observations,—we may say he jealously estimated their weight, for it must be remembered that he was in no way interested in securing a greater or less result, while he was greatly interested in so stating the value of his results that those who might succeed him in the inquiry should not detect any serious error in his estimate. But his opinion of the probable degree of error in his observations was such as scarcely to affect to an appreciable extent the statements we have made above. Taking Zöllner's lowest estimate of Jupiter's brightness, that statement remains appreciably correct.

And next as to corroborative evidence.

It happens that we have a very delicate means of measuring the degree of Jupiter's luminosity, as compared with that of other orbs similarly placed. For

his satellites pass across his face, and nothing can be easier than to observe whether they appear darker or brighter than his surface.

It was an observation such as this which Mr. Lassell had made on the night when he noticed the ruddiness of Jupiter's great medial belt. By a singular chance Father Secchi made a similar observation during *his* researches, and the reader will see, when we have quoted the narratives of both these observers, that the comparative darkness of all four satellites will have been established. 'The fourth satellite,' says Lassell, 'has begun again for a season to cross the planet's disc, and I have looked out for opportunities of observing its passages, and was favoured on the night of the 30th December last by witnessing a part of its passage under circumstances more than usually propitious. On its first entrance it was scarcely to be distinguished from the edge, not appearing at all as the others do, as a round bright spot. As it advanced it grew gradually manifestly darker than the surface of the planet, and by the time it had advanced a fourth of the way across it had become a very dark if not a *black* spot—so dark, indeed, that if I had looked at Jupiter without knowing anything of the positions of his satellites, I should have said that a *shadow* (of a satellite) was passing. I remember having seen the like phenomenon many years ago; but my impression is that I had never seen the disc of the satellite so near to absolute blackness before. Of course it is only by contrast that it can possibly so appear; and we have in this fact a striking proof of

the exceeding brilliancy of the surface of the planet. In the same way the solar spots, if not surrounded by the marvellous splendour of the sun's surface, would doubtless appear as brilliant objects.'

Next let us hear Secchi's account. 'On the evening of February 3rd,' he says, 'I observed the transit of the third satellite and that of its shadow. The satellite seemed almost black when it was upon the middle of the planet's disc, and notably smaller than its shadow, which was visible at the same time; one would have estimated it at only one-half. In approaching the edge the satellite disappeared, and reappeared soon after, close by the edge, but as a bright point. This fact is not a new one for the other satellites, but for the third it is unique. This result shows also the great difference of luminosity at the centre and near the edge of the planet, a difference already confirmed by photography.'

It is hardly necessary to point out how strikingly these facts illustrate and confirm Dr. Zöllner's observations. But they also supply fresh evidence of a very interesting nature.

Although a part of the difference dwelt on in Secchi's closing words may be ascribed to the oblique incidence of the light near the planet's edge, yet it does not appear to me that the whole difference can be thus explained. A difference so great that a satellite appears as a bright point close by the planet's edge, and almost black near the middle of the disc, suggests that the light near the edge is not reinforced by the inherent luminosity of our theory, that luminosity

adding only to the brightness of the central parts of the disc. I would not insist too strongly on this inference, because the darkening due to oblique incidence is, under certain circumstances, very obvious to direct observation. But it seems to us that a portion of the difference should be referred to the inherent luminosity of the central parts of the disc. This being admitted, it would follow that the real solid globe of the planet is much smaller than the globe measured by astronomers; and that, therefore, instead of that amazingly small density which is so perplexing a feature of the planet's physical condition, Jupiter's globe may have a density equalling or exceeding that of the earth.

And after all, let us remember that the theory that Jupiter is an intensely heated globe—a theory to which we have been led by the consideration of many observed facts, and which in its turn suggests very satisfactory explanations of other observed facts—would merely show that, as Jupiter and Saturn hold an intermediate position between the sun and the minor planets in respect of size, so those giant orbs hold a corresponding position in respect of inherent heat. Roughly speaking, the earth is 8,000 miles, the sun 840,000 miles, in diameter, and Jupiter, with his diameter of 82,000 miles, comes midway between these orbs. Now, the sun is at a white heat, and the earth gives out only what is called obscure heat; and if Jupiter's globe is at a red heat, he again comes midway between the sun and the earth.

We should be led by the theory here maintained to regard the major planets which travel outside the zone of asteroids as in a sense secondary suns. So viewed, they could not be regarded as orbs fit for the support of living creatures. Yet, as each of them is the centre of a scheme of dependent worlds, of dimensions large enough to supply room for many millions of living creatures, we should not merely find a *raison d'être* for the outer planets, but we should be far better able to explain their purpose in the scheme of creation than on any theory hitherto put forward respecting them. Jupiter as an abode of life is a source of wonder and perplexity, and his satellites seem scarcely to serve any useful purpose. He appears as a bleak and desolate dwelling-place, and they together supply him with scarcely a twentieth part of the light which we receive from our moon at full. But regarding Jupiter as a miniature sun, not indeed possessing any large degree of inherent lustre, but emitting a considerable quantity of heat, we recognize in him the fitting ruler of a scheme of subordinate orbs, whose inhabitants would require the heat which he affords to eke out the small supply which they receive directly from the sun. The Saturnian system, again, is no longer mysterious when thus viewed. The strange problem presented by the rings, which actually conceal the sun from immense regions of the planet for years together in the very heart of the winter of those regions, is satisfactorily solved when the Saturnian satellites are regarded as the abodes of life, and Saturn

himself as the source of a considerable proportion of their heat-supply. I do not say that, in thus exhibiting the Jovian and Saturnian systems in a manner which accords with our ideas respecting the laws of life in the universe, I have given irrefragable testimony in favour of my theory. That theory must stand or fall according to the evidence in its favour or against it. But so long as men believe that there is design in the scheme of the universe, they will be readier to accept conclusions which exhibit at once the major planets and their satellites as occupying an intelligible position in that scheme, than views which leave the satellites unaccounted for, and present the giant planets themselves as very questionable abodes for any known orders of living creatures.

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LIFE IN MARS.

DURING the month of May 1871 a somewhat unusual grouping of the planets was witnessed. Mars, Venus, and Jupiter, the three most conspicuous of these wandering orbs, were visible at the same time. Venus and Jupiter were in the west, where they shone conspicuously as evening stars. Nor was it difficult to distinguish between these rival orbs, for they passed each other on the 12th of May, Jupiter moving sunwards. Throughout the month Venus

was increasing in lustre, while Jupiter was diminishing—less markedly, indeed, but still perceptibly. The observer had a good opportunity of comparing the splendour of these planets, the two brightest orbs in the heavens—Venus, splendid because so near to us and to the sun, Jupiter less brilliantly illuminated by the solar rays, and lying at a distance from us enormously exceeding that of Venus, but enabled, by his vast bulk and by the high reflective power of his surface, to send to us an amount of light only inferior to that which we receive (under the most favourable conditions) from Venus.

But in the meantime, much farther towards the south, the ruddy planet Mars was conspicuous. He had returned to our neighbourhood, after an absence of two years, and was now about to urge his way eastwards among the stars, over the constellation Leo—over the Virgin, outshining the brilliant Spica which marks the wheat-ear in her hand, and thence—his lustre waning—through the Scales, and past his rival as a red star, Antares, or the Scorpion's Heart.

It may prove interesting to consider a few of the facts which astronomers have taught us about the Planet of War. For of all the planets, he is the one they can study best. He does not, indeed, come so near to us as Venus, nor does he, in the telescope, present so noble an appearance as Jupiter. Venus outshines him in the heavens, and Jupiter seems to show more interesting details in the telescopic field. Yet we see Mars, in reality, far better than either of those

two planets. If ever we are to recognise the signs of life in any of the orbs which people space, it will be in Mars that such signs will be first traced. As Venus comes near to us she assumes the form of the crescent moon, we have but a foreshortened view of a portion of her illuminated hemisphere, and her intensely bright light defeats the scrutiny of the most skilful observer. At the time of her nearest approach, she is lost wholly to our view in the splendour of the solar rays, her unilluminated or night hemisphere being directed also towards us. With Jupiter, the case is different. When at his nearest, he is seen under most favourable conditions, and the enormous dimensions of his belts render them very obvious and very beautiful features for the scrutiny of the telescopist. But then he is some 370 millions of miles from us at such a time, whereas Mars, when most favourably placed for telescopic study, is but 37 millions of miles away. A square mile on the surface of Mars would appear a hundred times larger than a square mile on the surface of Jupiter, supposing both planets studied when at their nearest. It is clear, then, that as respects surface-details, Mars is examined under much more favourable conditions than the giant planet Jupiter.

But here the question is naturally suggested, whether our own moon, which is but a quarter of a million of miles from us, ought not first to be examined for signs of life, or, at least, of being fitted for the support of life. When the telescope was first invented, it is certain that astronomers were more hopeful of recog-

nising such signs in the moon than in any other celestial body. As telescopes of greater and greater power were constructed, our satellite was searched with a more and more eager scrutiny. And many a long year elapsed before astronomers would accept the conclusion that the moon's surface is wholly unfitted for the support of any of those forms of life with which we are familiar upon earth. That the belief in lunar men prevailed in the popular mind long after astronomers had abandoned it, is shown by the eager credulity with which the story of Sir John Herschel's supposed observations of the customs and manners of the Lunarians was accepted amongst even well-educated men. Who can forget the gravity with which that most amazing hoax was repeated in all quarters? It was, indeed, ingeniously contrived. The anxiety of Sir John Herschel to secure the assistance of King William, and the care with which our 'sailor-king' inquired whether the interests of nautical astronomy would be advanced by the proposed inquiries; the plausible explanation of the mode of observation, depending, we were gravely assured, upon the *transfusion* of light; the trembling anxiety of Herschel and his fellow-workers as the moment arrived when their search was to commence; the flowers, resembling poppies, which first rewarded their scrutiny; and the final introduction upon the scene of those winged beings—not, strictly speaking, *men*, nor properly to be called angels — to whom Herschel assigned the generic appellation, *Vespertilio Homo*, or Bat-men. All these things, and many

others equally amusing, were described with marvellous gravity, and with an attention to details reminding one of the descriptions in 'Gulliver's Travels.' One can hardly wonder, then, that the narrative was received in many quarters with unquestioning faith, nor, perhaps, even at the simplicity with which (as Sir John Herschel himself relates) well-meaning persons planned measures for sending missionaries 'among the poor benighted Lunarians.'

Yet astronomers have long known full certainly that no forms of life such as we are familiar with can exist upon the moon. They know that if our satellite has an atmosphere at all, that atmosphere must be so limited in extent that no creatures we are acquainted with could live in it. They know that she has no oceans, seas, rivers, or lakes, neither clouds nor rains, and that if she had there would be no winds to waft moisture from place to place, or to cause the clouds to drop fatness upon the lunar fields. They know also that the moon's surface is subjected alternately to a cold far more intense than that which binds our arctic regions in everlasting frost, and to a heat compared with which the fierce noon of a tropical day is as the freshness of a spring morning. They search over the lunar disc for the signs of volcanic action only, feeling well assured that no traces of the existence of living creatures will ever be detected in that desolate orb.

But with Mars the case is far otherwise. All that we have learned respecting this charming planet leads

to the conclusion that it is well fitted to be the abode of life. We can trace, indeed, the progress of such changes as we may conceive that the inhabitants of Venus or of Mercury must recognise in the case of our own earth. The progress of summer and winter in the northern and southern halves of the planet, the effects due to the progress of the Martial day, from sunrise to sunset—nay, even hourly changes, corresponding to those which take place in our own skies, as clouds gather over our continents, or fall in rain, or are dissipated by solar heat: such signs as these that Mars is a world like ours can be recognised most clearly by all who care to study the planet with a telescope of adequate power.

As regards the atmosphere of Mars, by the way, the earliest telescopic observers fell into a somewhat strange mistake. For, noticing that stars seemed to disappear from view at some considerable distance from the planet, they assigned to the Martial atmosphere a depth of many hundreds of miles,—we care not to say how many. More careful observation, however, showed that the phenonemon upon which so much stress had been laid was merely optical. Sir J. South and other observers, carefully studying the planet with telescopes of modern construction, have been able to prove abundantly that the atmosphere of Mars has no such abnormal extension as Cassini and others of the earlier telescopists had imagined.

The early observations made on the polar snows of Mars were more trustworthy. Maraldi found that at

each of two points nearly opposite to each other on the globe of the planet, a white spot could be recognised, whose light, indeed, was so brilliant as to far outshine that emitted by the remainder of the disc. The idea that these white spots correspond in any way to the polar snows on our own earth does not seem to have occurred to Maraldi. Yet he made observations which were well calculated to suggest the idea, for he noticed that one of the spots had at a certain time diminished greatly in size. Instead, however, of ascribing this change to the progress of the Martial seasons, he was led to the strange conclusion that the white spot was undergoing a process of continuous decrease, and he even announced the date when, as he supposed, it would finally disappear.

No such disappearance took place, however. When Sir W. Herschel began his series of observations upon Mars, more than half a century later, the spots were still there. The energy of our great astronomer did not suffer these striking features to remain long unexamined. Searching, as was his wont, after terrestrial analogies—or, at least, analogies depending on known facts—he was quickly led to associate the white spots with our arctic regions. It would follow, of course, that in the summer months of either Martial hemisphere, the snow-cap would be reduced in size, while in the winter it would attain its greatest dimensions. Sir W. Herschel found this to be the case, and he was able to show that the changes which Maraldi had interpreted as suggesting the eventual disappearance of one

of the bright spots, were due to the progress of the Martial summer. Precisely as in our summer months, those who voyage across the Atlantic may sail in far higher latitudes than they could safely venture to traverse in winter, so in Mars the polar ice and snow is limited within a far narrower region in summer than in winter.

But after all (it may be urged), to suppose that these two bright spots are formed in reality of ice and snow is rather venturesome. Might we not imagine that some other material than water is concerned in the observed changes? What reason have we for inferring that the same elements that we are familiar with exist out yonder in space?

The answer to these questions,—or, rather, the answers, for we have to do with a whole series of facts, dovetailing in the most satisfactory manner into each other,—will be found full of interest.

We all know that Mars shines with a ruddy light. He is, indeed, far the ruddiest star in the heavens: Aldebaran and Antares are pale beside him. Now, in the telescope the surface of Mars does not appear wholly red. We have seen that at two opposite points his orb exhibits white spots. But, besides these regions, there are others which are not red. Dark spaces are seen, sometimes strangely complicated in figure, which present a well-marked tinge of greenish blue. Here, then, we have a feature which we should certainly expect to find if the polar spots are really snow-caps; for the existence of water in quantities sufficient to

account for snow regions covering many thousand square miles of the surface of Mars would undoubtedly lead us to infer the existence of oceans, and these oceans might be expected to resemble our own oceans in their general tint. According to this view, the dark greenish-blue markings on Mars would come to be regarded as the Martial seas.

If this be the case, then I may note in passing that the seas of Mars cover a much smaller proportion of his surface than those of our own earth, the extent of our seas being to that of our continents about the proportion of 11 to 4: in Mars the land and sea surfaces would seem to be nearly equal in extent. The seas in Mars are also very singularly shaped. They run into long inlets and straits; many are bottle or flask-shaped—that is, we see a somewhat rounded inland sea connected with what must be called the main ocean by a narrow inlet; and further it would seem as though oceanic communication must be far more complete in Mars (notwithstanding the relative smallness of his ocean surface) than on our own earth. One could travel by sea between all parts of Mars—with very few exceptions—the long inlets and the flask-shaped seas breaking up his land surface much more completely than the actual extent of water would lead us to infer. It may be supposed that on the other hand land communication is far more complete in the case of Mars than in that of our own earth. This is, indeed, the case, insomuch that such Martialists as object to sea travelling (and we can scarcely suppose sea-sickness

to be a phenomenon peculiar to our own earth) may very readily avoid it, and yet not be debarred from visiting any portion of their miniature world, save one or two extensive islands. Even these are separated by such narrow seas from the neighbouring continents, that we may regard it as fairly within the power of the Martial Brunels and Stephensons to bridge over the intervening straits, and so to enable the advocates of land-voyaging to visit these portions of their planet. This view is encouraged by the consideration that all engineering operations must be much more readily effected in Mars than on our own earth. The force of gravity is so small at the surface of Mars that a mass which on the earth weighs a pound, would weigh on Mars but about six and a quarter ounces, so that in every way the work of the engineer, and of his ally the spadesman, would be lightened. A being shaped as men are, but fourteen feet high, would be as active as a man six feet high, and many times more powerful. On such a scale, then, might the Martial navvies be framed. But that is not all. The soil in which they would work would weigh very much less, mass for mass, than that in which our terrestrial spadesmen labour. So that, between the far greater powers of Martial beings, and the far greater lightness of the materials they would have to deal with in constructing roads, canals, bridges, or the like, we may very reasonably conclude that the progress of such labours would be very much more rapid, and their scale very much more important than in the case of our own earth.

But let us return to our oceans, remembering that at present we have not proved that the dark greenish-blue regions we have called oceans really consist of water.

It might seem hopeless to inquire whether this is the case. Unless the astronomer could visit Mars and sail upon the Martial seas, he could never learn—so at a first view one might fairly judge—whether the dark markings he chooses to call oceans are really so or not.

But he possesses an instrument which can answer even such a question as this. The spectroscope, the ally of the telescope—useless without the latter, but able to tell us much which the most powerful telescope could never reveal—has been called in to solve this special problem. It cannot, indeed, directly answer our question. It cannot so analyse the light from the greenish markings as to tell us the nature of the material which emits or reflects to us that peculiarly tinted light. But the astronomer and physicist is capable of reasoning as to certain effects which must necessarily follow if the Planet of War have oceans and polar snow-caps, and which could not possibly appear if the markings we call oceans were not really so, nor the white spots at the Martial poles really snow-caps. Extensive seas in one part of the planet, and extensive snow regions in another, would imply, in a manner there could be no mistaking, that the vapour of water is raised in large quantities from the Martial oceans to be transferred by Martial winds to polar regions, there to fall in snow-showers. It is this aqueous vapour in the

Martial atmosphere that the spectroscope can inform us about. Our spectroscopists know quite well what the vapour of water is capable of showing in the rainbow-tinted streak which is called the spectrum. When white light is caused to shine through a sufficient quantity of the vapour of water, the rainbow-tinted streak forming the spectrum of white light is seen to be crossed by certain dark lines, whose position and arrangement there is no mistaking. Now the light we get from Mars is reflected sunlight, but it is sunlight which has been subjected to more than reflection, since it has passed twice through the depths of the Martial atmosphere, first while passing to his surface, and secondly while leaving that surface on its voyage towards ourselves. If that double passage have carried it through the vapour of water, the spectroscope will certainly tell us of the fact.

Let us see how this problem was dealt with by our most skilful spectroscopist, Dr. Huggins, justly called the Herschel of the spectroscope. The following account is an epitome of his own narrative :—'On February 14 he examined Mars with a spectroscope attached to his powerful eight-inch refractor. The rainbow-coloured streak was crossed, near the orange part, by groups of lines agreeing in position with those seen in the solar spectrum when the sun is low down and so shines through the vapour-laden lower strata of our atmosphere. To determine whether these lines belonged to the light from Mars or were caused by our own atmosphere, Dr. Huggins turned his spectroscope

towards the moon, which was at the time nearer to the horizon than Mars, so that the lines belonging to our own atmosphere would be stronger in the moon's spectrum than in that of the planet. But the groups of lines referred to were not visible in the lunar spectrum. It remained clear, therefore, that they belonged to the atmosphere of Mars, and not to our own.'

This observation removes all reasonable doubt as to the real character as well of the dark greenish-blue markings as of the white polar caps. We see that Mars certainly possesses seas resembling our own, and as certainly that he has his arctic regions, waxing and waning, as our own do, with the progress of the seasons. But in fact Dr. Huggins's observation proves much more than this. The aqueous vapour raised from the Martial seas can find its way to the Martial poles only along a certain course—that is, by traversing a Martial atmosphere. Mars certainly has an atmosphere, therefore, though whether the constitution of that atmosphere exactly resembles that of our own air is not so certainly known. On this point the spectroscope has given no positive information, yet it allows us to draw this negative inference—that, inasmuch as no new lines are seen in the spectrum of the planet, it would seem likely that no gases other than those existing in our own atmosphere are present in the atmosphere of Mars.

But we are naturally led to inquire whether the phenomena which our meteorologists have to deal

with—clouds, fog, and mist, wind-storms and rain-storms—can be recognised, either directly or in their effects, when Mars is studied with the telescope. The answer is full of interest. We have been able to learn much respecting the meteorology of this distant world. In the first place, we see that at times the features of his globe—those well-recognised markings which indicate the figure of oceans and continents—are hidden from view as if by clouds. A whitish light replaces the well-marked red colour of the continents or the equally well-marked green-blue tint of the oceans. But more. We can at times actually watch the gradual clearing up of the Martial skies, for we can see the whitish region of light gradually growing smaller and smaller, the features it had concealed coming gradually into view. On one occasion Mr. Lockyer was observing Mars with an excellent telescope, six inches in aperture, when he became aware that a change of this sort was in progress. A certain well-known sea was partially concealed from view by a great cloud-mass spreading over many thousand square miles of the Martial surface. But as the hours passed, the clouds seemed to be melting away, whether by the sun's heat or because they had fallen in rain was, of course, not determinable. When Mr. Lockyer ceased observing for the evening—at about half-past eleven—a large proportion of the sea before concealed had come into view. But on the same night, the eagle-eyed Dawes, the prince of modern telescopists, as he has been called, was also

studying the Planet of War. Waiting until the outlines of the oceans and continents had become clearly discernible, he made ("in the wee sma' hours ayont the twae") an excellent drawing of Mars. When this was compared with the drawing made at an earlier hour by Mr. Lockyer, it was seen that the clouds which had concealed a portion of the planet had, at the later hour, passed completely away, insomuch that the whole of the shore-line, which was at first concealed, had been restored to view. And it is worthy of notice, that, referring these events to Martial time, the cloudy weather in this part of Mars appears to have occurred in the forenoon, the midday hour (as often happens on earth) bringing clear weather, which would seem to have lasted until the Martial afternoon was far advanced.

But we can also learn something of the general progress of the weather during a Martial day. It would seem that, as a rule, the Martial mornings and evenings are misty. This, at least, seems the most satisfactory explanation of the whitish light which is usually seen all round the planet's disc; for the parts of the planet which lie near the edge of the disc are those where the sun is low—that is, where it is either morning or evening out yonder on Mars. The presence, therefore, of this whitish light would seem to indicate misty mornings and misty evenings in Mars.

It seems clear too that—as with ourselves—winter is more cloudy than summer; for it is always noticed that near the Martial solstices the markings on that

half of the planet where winter is in progress are very indistinctly seen, a whitish light sometimes replacing the red and green markings altogether in these regions. On the contrary, the regions where summer is in progress, are generally very well seen.

The reader will infer from what has been said on these points, that the study of Mars cannot be carried on very rapidly by our astronomers; for, in the first place, Mars only returns to our midnight skies at intervals of more than two years, and he remains but for a short time favourably placed for observation. Then one half of his surface only can be seen at a time, and nearly one half even of that hemisphere is commonly concealed by clouds, which also extend all round the disc, so that perhaps but about one-eighth of the planet's surface can be favourably studied. When we add to these considerations the circumstance that not one night out of ten in our climate—or, perhaps, in any—is well suited for the use of powerful telescopes, while even favourable nights cannot always be devoted to the study of Mars (other celestial objects often requiring special attention), it will be understood that the progress of discovery has not been so rapid as, at a first view, might be expected. When we are told that more than two centuries have elapsed since the telescopic study of Mars began, it seems as though ample time had been given for research; but the time which has been actually available for that purpose has been far more limited than that estimate would imply.

And now, returning to the consideration of the

probable condition of Mars, with respect to those circumstances which we regard as associated with the requirements of living creatures, let us briefly inquire how far we can determine aught as to the geological structure of the planet. Here the spectroscope cannot help us. The telescope, and such reasoning as may fairly be applied to the relations already dealt with, must here be our main resource. We see, then, that the land regions of the planet present a ruddy tinge. Sir John Herschel has suggested, and I am not here concerned to deny, that this is probably due to the ochreish nature of the soil. The planet, in fact, is to be regarded as perhaps passing through a geological era resembling that through which our own earth was passing when the Old Red Sandstone constituted the main proportion of her continents. But it certainly must be admitted, as a remarkable circumstance, that we can trace no signs of extensive forests in Mars, nor any such appearances as we should imagine that our prairies must present to telescopists in Venus or Mercury. One is almost invited to adopt the bizarre notion of that French astronomer who suggested that vegetation on Mars is red instead of verdant—that in this distant and miniature world the poet may sing of spring, more truly than our terrestrial poets, that

> She cometh blushing like a maid.

As respects the absence of forests, we may perhaps find a sufficient explanation in the fact that lofty trees would exist under somewhat unfavourable conditions in Mars; for gravity being so much less than on our own earth,

the stability of objects having equal dimensions would be correspondingly reduced. On the other hand, the winds which blow in Mars are probably, as Professor Phillips has pointed out, exceedingly violent; so that to quote a striking paper which appeared last year in the *Spectator* (in a review of my 'Other Worlds'), 'if currents of air in Mars are of more than usual violence, while the solidifying force of friction which resists them is much smaller than here, it may be a reasonable inference that "natural selection" has already weeded out the loftier growing trees, which would stand less chance in encounters with hurricanes than our own.' The absence of prairies is not so easily explained, however; and the idea is in fact suggested that some of those regions which have hitherto been included among the Martial seas, are in reality regions richly covered with verdure. Nor are we wholly without evidence in favour of this view; for there is a certain very wide tract in Mars respecting which the late Mr. Dawes remarked to me that he found himself greatly perplexed. 'At times,' he said, 'I seem to see clear traces of seas there; but at other times I find no such traces.' These regions have accordingly been regarded as extensive tracts of marsh land. But the idea seems at least worth considering that they may be forest regions or extensive prairies.

There must needs be rivers in Mars, since the clouds, which often cover whole continents, must pour down enormous quantities of rain, and this rain-fall must find a course for itself along the Martial valleys to the sea.

Indeed, we can have no doubt that Mars has been the scene of those volcanic disturbances to which our own mountains, hills, valleys, and ravines owe their origin. The very existence of continents and oceans implies an unevenness of surface which can only be explained as the effect of subterranean forces. Volcanoes must exist, then, in Mars; nor can his inhabitants be wholly safe from such earthquake throes as we experience. It may be questioned, indeed, whether subterranean forces in Mars are not relatively far more intense than in our own earth,—the materials of which the planet is formed being not only somewhat less massive in themselves, but also held down by a gravity much less effective.

It would seem, also, that the Martial oceans must be traversed by currents somewhat resembling those which traverse our own oceans. There is, indeed, a very marked difference between our seas and those of Mars. For apart from the circumstance that the terrestrial oceans cover a much greater proportion of the earth's surface, the Martial seas are scarcely traversed by appreciable tides. Mars has no moon to sway his ocean waters, and though the sun has power over his seas to some slight extent, yet the tidal waves thus raised would be very unimportant, even though the seas of Mars were extensive enough for the generation of true tidal oscillations. For, in the first place, Mars is much farther from the sun, and the sun's action is correspondingly reduced—it is reduced, in fact, on this account alone more than threefold. But further, Mars is much smaller than the earth, and the dimensions of

our earth have much to do with the matter of the sun's tide-raising power. Every one knows how the explanation of the tides runs in our books of astronomy and geography. The sun is nearer to the water turned directly towards him than he is to the centre of the earth; he therefore draws that water away from the earth, or in other words raises a wave: but again, says the explanation, the sun is nearer to the earth's centre than to the water on the side turned away from him, and therefore he draws the earth away from that water, or a wave is raised on the further as well as on the nearer side of the earth. If the earth were smaller, the sun would not be so much nearer to the water turned towards him, nor so much farther from the water turned away from him—so that both waves would be reduced in dimensions. Applying this consideration to the case of Mars, whose orb is much smaller than the earth's, we see that any tidal wave raised by the sun in Martial seas must needs be of very small dimensions.

But the existence of ocean currents appears to depend very little on the presence of tidal waves. In the Mediterranean Sea, the Red Sea, and the Baltic Sea well-marked currents exist, although the tidal wave scarcely affects these seas. Sea-currents would indeed seem to be due to the effects of evaporation taking place extensively over certain portions of the sea surface; and we know that evaporation must proceed very freely in the case of the seas of Mars, since clouds form so marked a feature of his atmospheric economy. We may conclude, therefore, that his seas are traversed

by currents, and further that most of those effects which our students of physical geography ascribe to ocean currents, take place also in the case of Mars.

Summing up the results here considered, we seem to recognise abundant reasons for regarding the ruddy planet which is now shining so conspicuously in our skies as a fit abode for living creatures. It would seem, indeed, unreasonable to doubt that that globe is habitable which presents so many analogies to our own, and which differs from our own in no circumstances that can be regarded as essential to the wants of living creatures.

(From the *Cornhill Magazine* for May 1871.)

A WHEWELLITE ESSAY ON THE PLANET MARS.

The planet Mars has returned to our nocturnal skies, after being unfavourably placed for rather more than two years. He now shines throughout the night as a ruddy star in the constellation Virgo—distinguished by his superior lustre, as well as by his colour and the steadiness of his light, from the leading brilliants of that constellation. Night after night, he will rise earlier, becoming towards July and August an evening star in the ordinary sense of that expression—for, strictly speaking, he is already an evening star.

When Mars was last in a favourable position for observation, I wrote an essay, entitled *Life in Mars*, describing the considerations which have led astro-

nomers to believe that in this planet conditions may prevail which would render life possible for such creatures as we are familiar with on earth. That essay dealt, in fact, with the arguments which would have been employed by Brewster in maintaining his position against a Whewell of the present day. I propose in the present essay to discuss certain considerations which point in a different direction, and would certainly not be left untouched by Whewell if he now lived, and sought to maintain his position against the believers in more worlds than one.

It is a little hard perhaps, that an attack should be made against the habitability of Mars; for, though we are in the habit of speaking somewhat confidently of life in other worlds, it is, as a matter of fact, in Mars alone that astronomers have hitherto recognised any approach to those conditions which we regard as necessary for the requirements of living beings. All that is known about Mercury and Venus, tends to the conclusion that very few of the creatures existing on our earth could live in either planet—and assuredly man is not among those creatures. It is not merely that in both these planets the average daily supply of heat is far greater than we could endure unscathed, but that from the pose of these planets—the slope of their axes to the level of their path—the supply of heat varies greatly in amount, so that at one time there is much more than even that average supply which *we* could not bear, and at another no heat is received at all for many days in succession, or else a supply so small in quantity that

beings like men would perish with the resulting cold. And when, passing beyond Mars, and traversing the wonderful ring of small planets, we come to Jupiter, where, so far as direct solar heat is concerned, we are assured that there is not a tithe of the supply which would be necessary for beings like ourselves. For the gap between Mars and Jupiter is quite unlike that which separates Mars from the earth, and the earth from Venus (referring of course to the paths of these bodies). From Mars to Jupiter is fully six times the distance from the earth to Mars, and the solar light and heat at Jupiter are reduced to less than the ninth part of the light and heat which are received by Mars. Of course Saturn, Uranus, and Neptune are still less fitted to be the abodes of creatures such as those which inhabit the earth.

Mars alone had given promise of habitability in the ordinary sense of the term. And the study of Mars had revealed many interesting results, apparently confirming in a striking manner the opinion that he is a 'miniature of our earth'—a globe resembling the earth in physical habitudes, and like her the abode of living creatures, amongst which may be races resembling man. We know that Mars is not so very much farther than the earth from the sun, as at a first view to dispose of all idea that he is inhabited. His year is not so much longer than ours as to render our conceptions of his seasons incompatible with the existence of vegetable life resembling that which exists on the earth. Then we know that his seasons resemble those of the earth in

their range: his arctic, temperate, and torrid zones occupy nearly the same relative portions of his globe as ours do. His day, again, only differs from the terrestrial day by about thirty-seven minutes. Water certainly exists on his surface, and the vapour of water is present in his atmosphere. Oceans and continents can be recognised on his globe—they have even been mapped and charted, and globes have been formed of the ruddy planet. The polar snow-caps of Mars can also be seen, and their increase and diminution with the varying seasons can be readily recognised. The signs of cloud and mist and rain, ocean-currents and air-currents, have also been traced. In fine, everything which one could hope to find as indicative of the habitability of so distant a world, has been seen in Mars; and accordingly it is not greatly to be wondered at if the theory that he is inhabited, and by beings not very unlike those existing on our earth, should have been comfortably accepted by most of those who have considered the subject.

Yet there has always been a serious difficulty in the way. Although the distance of Mars from the sun is not so much in excess of the earth's as to *compel* us to forego the idea that he is suitably warmed and lighted (reference being always made to the wants of such creatures as we are familiar with), yet there is a sufficient discrepancy to render it somewhat surprising that the meteorological conditions on Mars should apparently resemble those on the earth very closely. This would not be the place for nice calculations, and

therefore I give results without entering into the details of the processes by which they have been obtained. It is the case, then, that the average daily supply of light and heat on Mars (square mile for square mile of his surface) is less than the supply on the earth in the proportion of two to five. When he is at his nearest to the sun, the daily supply amounts to rather more than a half that received by the earth; but when he is at his farthest, the daily supply falls to little more than one-third of the earth's.

This is a very serious deficiency when rightly understood. We must not content ourselves by comparing it to the difference between the heat of a winter day and a summer day. We often have to endure for several days in succession a much greater degree of cold than would follow from the mere reduction of the sun's ordinary heat to one-third its present value, and the deficiency is not destructive to life. But it would be quite another matter if the whole supply of light and heat to the earth were reduced in this proportion. It must be remembered that to that supply we owe the continuance of all the forms of force, including vitality, on the whole earth. 'The sun's rays,' said John Herschel, in 1833,* 'are the ultimate source of almost every motion which takes place on the surface of the earth. By its heat are produced all winds and those disturbances in the electric equilibrium of the atmosphere which give rise to the phenomena of

* Before the notion had suggested itself to Stephenson, to whom it is commonly referred.

lightning, and probably, also, to terrestrial magnetism and the aurora. By their vivifying action vegetables are enabled to draw support from organic matter; and become in their turn the support of animals and man, and the source of those great deposits of dynamical efficiency which are laid up for human use in our coal strata. By them the waters of the sea are made to circulate in vapour through the air, and irrigate the land, producing springs and rivers. By them are produced all disturbances of the chemical equilibrium of the elements of nature, which, by a series of compositions and decompositions, give rise to new products, and originate a transfer of materials. Even the slow degradation of the solid constituents of the surface in which its chief geological change consists, is almost entirely due, on the one hand, to the abrasion of wind or rain and the alteration of heat and frost, on the other, to the continual beating of sea-waves agitated by winds, the results of solar radiation.'

What would happen if the source of all these processes, of every form, in fact, of force existing and acting on the earth were to lose more than one-half of its power? We can answer this question best by another. What would happen if the engine working a mighty system of machinery were deprived of more than one-half of its due supply of fuel? The engine might continue to work, but it would no longer work efficiently. The machinery would no longer serve its purpose. And in like manner the great machinery which is maintained by solar action on the earth, would no longer

subserve *its* purpose—or if the vocabulary of teleology must be eschewed, this great machinery would no longer do what it is actually doing, would no longer maintain active life upon the earth. If life still continued it would be sluggish, little more, in fact, than living death.

And if the failure of the solar supply at this present time would lead to such a result, how much more completely fatal to the existence of all such life as we now see upon the earth, would have been a defalcation of solar light and heat during the long-past ages when so many forms of force were stored up. To take one such form alone, and to consider it only as it affects the requirements of our own country—'the "deposits of dynamical efficiency" laid up in our coal strata are simply,' as Tyndall tells us, 'the sun's rays in a potential form. We dig from our pits annually a hundred million tons of coal, the mechanical equivalent of which is of almost fabulous vastness. The combustion of a single pound of coal in one minute is equal to the work of three hundred horses for the same time. It would require one hundred and eight millions of horses working day and night with unimpaired strength for a year to perform an amount of work equivalent to the energy which the Sun of the Carboniferous Epoch invested in one year's produce of our coal-pits. The further we pursue this subject, the more its interest and wonder grow upon us. Here we find the physical powers derived from the sun introducing themselves at the very root of the question of vitality. We find in solar light and heat the very mainspring of vegetable life.'

If Mars then not only receives day by day a much smaller supply of light and heat than our earth, but has been similarly circumstanced during all those past ages which supply the facts studied by geologists, what opinion must we form as to his present fitness to be the abode of creatures like those which exist upon our earth? It appears to us that there can be but one answer to this question. Our only doubt must depend on our acceptance of the opinion on which the question is based. If in any way the supply of heat has been increased, or—which amounts to the same thing—if a greater portion of the direct supply has been stored up, then, and then only can we regard Mars as a suitable abode for living creatures like those on the earth. For we may dismiss the supposition that the inherent heat of Mars's globe is such as to compensate for a deficiency in the supply of solar heat. So far is this from being at all probable, that on the contrary an additional difficulty is introduced by the consideration that in all reasonable likelihood Mars must have parted with a very much greater proportion of his inherent heat than our earth. His globe is very much smaller than that of the earth, and the total quantity of matter contained in it is little more than one-ninth of the matter contained in the earth's globe. Now, it is known that of two bodies equally heated, the smaller cools more rapidly than the larger. And certainly we have no reason to believe that at any epoch Mars was hotter than the earth at the same epoch. We should infer, indeed, that Mars was always much the less

heated body. For according to the most generally received explanation of the original intense heat of the planet, such heat had its origin in the rush of matter drawn in by the attractive might of the aggregation which was, so to speak, the embryon of the planet. Thus the smaller planets, which must necessarily have had less attractive energy than the larger, would impart a less velocity to the inrushing matter, and therefore would be less intensely heated. On all accounts it would follow that Mars is, at the present time, a much colder body than the earth.

Our sole resource, therefore, if we are to adopt the theory that the climate of Mars resembles that of the earth, is to assume that there is some peculiarity in his atmosphere by which it is enabled to retain a larger proportion of the heat received from the sun than happens in the case of our own atmosphere. If we are further to assume that the constitution of the atmosphere resembles that of our air—and no other assumption is compatible with the belief that creatures such as we are familiar with can exist in Mars—we must assume that the Martial atmosphere is much more dense than our own. We need not enter here into the considerations on which this inference is based. Let it suffice to remark that there is a steady decrease of warmth with elevation in all parts of the earth, this decrease being unquestionably due to the greater tenuity of the air in high regions. And it is certain that if the density of the air were in any way increased, there would be a corresponding increase of warmth.

But when we apply this consideration to the case of Mars we find a difficulty in the disproportionate amount of atmosphere which must be assigned to this small planet. It seems a very natural and probable assumption that every planet would have an atmosphere proportional in quantity to the quantity of matter in the planet. Thus since the mass of Mars is but one-ninth of the earth's mass, we should infer that his atmosphere amounted in quantity to but one-ninth part of the earth's atmosphere. Of course we could not lay any stress on such an assumption; but it must be regarded as more probable, on *à priori* grounds, than any other. This would leave Mars with much less air over each square mile of his surface than there is over each square mile of the earth's surface: for the surface of Mars is much greater than a ninth part of the earth's; it is, in fact, between a third and a fourth of the earth's surface. But this is not all; not only (on the assumption we are dealing with) would there be much less air over each mile of the surface of Mars but this smaller quantity of air would be much less strongly attracted towards the surface of the planet. For, owing to his small bulk and the comparative lightness of the materials of which he is constructed, Mars exerts less than two-fifths of the attractive force which our earth exerts. A mass which, on our earth, would weigh a pound, would on Mars weigh little more than six ounces; and the atmospheric pressure would be correspondingly reduced, even though Mars had as much air above each square mile of his surface as there

is above each square mile of the earth's. Such an amount of air would be twice as much as we should infer from the mass of Mars, and we should require five times as much air only to have an atmosphere as dense as our own at the sea level. An atmosphere about twice as dense as this would perhaps give a climate as mild, on the average, as that of our earth. But it seems rather a daring assumption to assign to Mars an atmosphere exceeding *ten* times in quantity what we should infer from the planet's mass.

It seems, on the whole, safer to abandon the theory that Mars is a suitable abode for such creatures as exist on the earth; and to try to explain observed appearances unhampered by a theory which after all is not in itself a probable one. For indeed we can employ in a very effective way against this theory a mode of argument which is commonly urged in its favour. It is reasoned that since the earth, the only planet we know, is inhabited, therefore probably the other planets are so. But we have seen that, so far as the evidence goes, all the other planets, save Mars alone, are probably not inhabited by beings such as those which exist upon the earth. Therefore, even on *à priori* grounds, it is more likely that Mars is similarly circumstanced; since there are six planets in favour of this inference, and only one, our earth, against it.

In resuming the inquiry, with the theory of Mars's habitability abandoned for the nonce, we must recall the facts which have been demonstrated respecting Mars, only we may now view them in a new light. We

remember that he has polar snow-caps; but we are no longer bound to regard these snow-covered regions as in any sense resembling our arctic regions. Again, the seas and oceans of Mars may be permanently frozen thoughout the greater part of their depth. The water-vapour which is certainly present in his atmosphere may be raised only by the midday sun, to be precipitated in early evening. Winds and currents may equally well prevail in a rare as in a dense atmosphere. The white masses which have been compared to clouds, and whose dissipation has been held to imply the downfall of rain on Mars, may not be rain-clouds, but snow-clouds; or, where there is no downfall, they may be not cumulus-clouds, but cirrus-clouds,—that is, not such clouds as are raised in our dense air near the sea-level by the sun's warmth, but such light fleecy clouds as are suspended high above the loftiest mountain summits.

It appears to us, indeed, that if we make any change at all in our views about Mars, we must make a great change. If we suppose the Martian air moderately dense, comparable in density at any rate with our own air, then since we know that considerable quantities of aqueous vapour are raised into that air, we seem compelled to conclude that there would be a precipitation of snow (under the circumstanceces already considered) which should keep the surface of Mars as permanently snow-covered as our mountain-heights above the snow-line. As this is not the case, for Mars is not a white planet, we *must* assume so great a rarity

of the Martial atmosphere that sufficient water-vapour can never be raised into that air to produce a permanent snow-envelope by precipitation. This view (on which we shall presently touch again) of course accords well with the *à priori* opinion respecting the Martian atmosphere referred to above. And therefore it seems to us manifestly the most probable and satisfactory course to assume that the Martian atmosphere bears about the same relation to ours in quantity which the mass of Mars bears to that of the earth. On this assumption it is easily shown that the atmospheric pressure on Mars corresponds to about four and a half inches of the mercurial barometer. We may take five inches as a fair probable estimate of the height of Martian barometric tubes, supposing there are any reasoning creatures on Mars who have made the same discovery as our terrestrial Torricelli.

At this stage it may be interesting to inquire whether the mere tenuity of the Martian air, on our assumption, would be a fatal objection to the theory that creatures like men can live on the planet. Could any man, for instance, exist for any length of time in an atmosphere corresponding in pressure to only four or five inches of the common barometer? or could any race of men, after a gradual process of acclimatisation, become enabled not merely to live in such an atmosphere but to thrive as a race, to undergo ordinary labours, to travel without being easily exhausted, and, if need were, to defend themselves against their enemies or from sudden natural dangers?

The experiment has never yet been tried. Nor is it easy to see how it could be. Aeronauts have reached a height where the atmospheric pressure has been reduced to below seven inches of the common barometer; but in attaining this height they were exposed to other effects than those due to the mere tenuity of the atmosphere. I refer here to the celebrated ascent by Coxwell and Glaisher, on July 17, 1862, when the enormous elevation of 37,000 feet was attained, or nearly two miles above the summit of the loftiest mountain of the earth. But, although the circumstances of such an ascent do not altogether correspond to those depending solely on atmospheric rarity, it is probable that the most remarkable effects result from this cause, and therefore it will be well to consider what happened to the aeronauts on this journey. 'Previous to the start,' says Flammarion, in a work edited by Mr. Glaisher, 'Glaisher's pulse stood at 76 beats a minute; Mr. Coxwell's at 74. At 17,000 feet, the pulse of the former was at 84; of the latter at 100. At 19,000 feet, Glaisher's hands and lips were quite blue, but not his face.' At this height the atmospheric pressure was reduced to about one-half the pressure at the sea-level; in other words, the pressure corresponded to about fourteen and a half inches of the mercurial barometer. After passing beyond this height, distressing symptoms were experienced by both aeronauts. 'At 21,000 feet, Glaisher heard his heart beating, and his breathing was becoming oppressed; at 29,000 feet he became senseless, and only returned to himself when

the balloon had come down again to the same level. At 37,000 feet Coxwell could no longer use his hands, and was obliged to pull the string of the valve with his teeth. A few minutes later he would have swooned away, and probably lost his life. The temperature of the air was at this time twelve degrees below zero.' This certainly does not suggest that life on the earth would be pleasant, if the air were reduced in quantity to that above the level reached by Coxwell and Glaisher on this occasion. But the barometer still stood nearly seven inches high when they began to descend, at which time Glaisher was nearly two miles above his fainting level, while Coxwell was all but powerless. And then it is to be remembered, as Flammarion well remarks, that in balloon ascents 'the explorer remains motionless, expending little or none of his strength, and he can therefore reach a greater elevation before feeling the disturbance which brings to a halt at a far lower level the traveller who ascends by the sole strength of his muscles the steep sides of a mountain.' What would be the state of a traveller having to exert himself in an atmosphere reduced to five-sevenths of the density of the air in which Coxwell was just able to save his own life and Glaisher's,—literally 'by the skin of his teeth'?

To show the effect of active exertion in increasing the unpleasant results of great atmospheric tenuity, we may quote the experience of De Saussure, in his ascent of Mont Blanc, noting however that recent Alpine travellers seem to have been more favoured, while the

guides would appear to have become more inured to the hardships of high places than they were in 1787. We learn that 'at 13,000 feet, upon the Petit-Plateau, where he passed the night, the hardy guides, to whom the previous marching was absolute child's play, had only removed five or six spades-full of snow in order to pitch the tent, when they were obliged to give in and take a rest, while several felt so indisposed that they were compelled to lie upon the snow to prevent themselves from fainting. The next day,' says De Saussure, 'in mounting the last ridge which leads to the summit, I was obliged to halt for breath at every fifteen or sixteen paces, generally remaining upright and leaning on my stock; but on more than one occasion I had to lie down, as I felt an absolute need of repose. If I attempted to surmount the feeling, my legs refused to perform their functions; I had an initiatory feeling of faintness, and was dazzled in a way quite independent of the action of the light, for the double crape over my face entirely sheltered the eyes. As I saw with regret the time intended for experiments on the summit slipping away, I made several attempts to shorten these intervals of rest. I tried, for instance, a momentary stoppage every four or five paces, instead of going to the limit of my strength; but to no purpose, as at the end of the fifteen or sixteen paces I was obliged to rest again for as long a time as if I had done them at a stretch. . . . The only thing which refreshed me and augmented my strength was the fresh wind from the north. When, in mounting, I had this in my face, and

could swallow it down in gulps, I could take twenty-five or twenty-six paces without stopping.'

It must not be overlooked, however, that some of the effects thus experienced appear to be due to the presence of impure air. For experiments made by De Saussure showed that air near the surface of snow contains less oxygen than the surrounding air; and Boussingault points out respecting 'certain hollows and enclosed valleys of the higher part of Mont Blanc—in the *Corridor*, for instance—that people generally feel so unwell when traversing it that the guides long thought this part of the mountain impregnated with some mephitic exhalation. Thus even now, whenever the weather permits, people ascend by the *Bosses* ridge, where a purer air prevents the physiological disturbances from being so intense.'

There are, indeed, parts of the earth where, at an elevation nearly as great as that at which De Saussure experienced such unpleasant effects, the inhabitants of considerable cities enjoy health and strength. As Boussingault well remarks, 'When one has seen the activity which goes on in towns like Bogota, Micuipampa, Potosi, &c., which have a height of from 8,500 feet to 13,000 feet; when one has witnessed the strength and agility of the torreadors in a bull fight at Quito (9,541 feet); when one has seen young and delicate women dance for the whole night long in localities almost as lofty as Mont Blanc; when one remembers that a celebrated combat, that of Pichincha, took place at a height as great as that of Monte Rosa

(15,000 feet), it will be admitted that man can become habituated to the rarefied air of the highest mountains.' These places are, however, tropical, and it is manifest that cold plays an important part in producing the unpleasant sensations which are experienced in elevated regions. Since in Mars (according to our present assumption) we have not only a much greater atmospheric rarity than at the highest peak of the Himalayas, but also a much greater degree of cold than at such a height even in high latitudes, it is manifest that absolute uninhabitability by human beings must result. Nay, since no living things except microscopic animalcules exist above certain elevations, or when a certain degree of cold is experienced, it remains clear that Mars cannot possibly be inhabited by creatures resembling any of the higher forms of living beings with which we are familiar on earth. 'Beyond the last stage of vegetation, beyond the extreme region attained by the insect and mammifers, all becomes silent and uninhabited,' says Flammarion,' 'though the air is still full of microscopic animalcules which the wind raises up like dust and which are disseminated to an unknown height.'

But the reader may be led to ask, at this stage, what is actually taking place in Mars when our astronomers perceive signs as of clouds forming and dissolving, of morning and evening mists, and other phenomena not compatible, it should seem, with the idea of extreme cold. Nay, it is to be remembered that even the presence of ice and snow implies the action of heat. 'Cold alone,'

says Tyndall, 'will not produce glaciers. You may have the bitterest north-east winds here in London throughout the winter without a single flake of snow. Cold must have the fitting object to operate upon, and this object—the aqueous vapour of the air—is the direct product of heat.' It is manifest, then, that the sun exerts enough heat on Mars to raise the vapour of water into the planet's atmosphere (as indeed spectroscopic analysis has taught us), and it is also clear that this vapour must be conveyed in some way to the Martian arctic regions, there to be precipitated in the form of snow. And then this difficulty is introduced: According to our ideas the whole surface of Mars is above the snow-line; any region on our earth where so great a degree of cold prevailed accompanied by so great an atmospheric tenuity would be far above the snow-line even at the equator. How is it then that the snow ever melts, as it manifestly does, since we can see the ruddy surface of the planet?

An explanation, first suggested, we believe, in Mr. Mattieu Williams's ingenious book called *The Fuel of the Sun*, removes this difficulty. The snow actually falling on Mars must be small in quantity, simply because the sun's heat is not competent to raise up any great quantity of water vapour. There cannot, then, be anything like the accumulation of snow which gathers in regions above our snow-line; but instead of this there must exist over the surface of Mars, except near the poles, a thin coating of snow, or rather there will be ordinarily a mere coating of hoarfrost. Now

the sun of Mars, though powerless to raise great quantities of vapour into the planet's tenuous atmosphere, is perfectly competent to melt and vaporize this thin coating of snow or hoarfrost. The direct heat of the sun, shining through so thin an atmosphere, must be considerable wherever the sun is at a sufficient elevation; and of course the very tenuity of the air renders vaporization so much the easier, for the boiling-point (and consequently all temperatures of evaporation at given rates) would be correspondingly lowered.* Accordingly, during the greater part of the Martial day the hoar frost and whatever light snow might have fallen on the preceding evening would be completely dissolved away, and thus the ruddy earth or the greenish ice-masses of the so-called oceans would be revealed to the terrestrial observer. We may picture the result by conceiving one of those Martial globes which Captain Busk has recently caused Messrs. Malby to make from my charts, to be first coated with thin hoarfrost, and then held before a fire just long enough to melt the hoarfrost on the part of the globe nearest to the fire,

* Amongst other disadvantages presented by Mars, regarded as an abode for beings like ourselves, is the circumstance that if his atmosphere be in proportion to his mass, as we have assumed, it must be impossible to boil food properly on the ruddy planet. For water would boil at a temperature about seventy degrees below our boiling point, so that it would barely be heated enough to parboil. A cup of good tea is an impossibility in Mars, and equally out of the question is a well-boiled potato. It does not make matters more pleasant that the tea-plant and the potato are impossible, of themselves, on Mars, and that therefore the possibility of boiling them may be regarded as a secondary consideration.

leaving the features of the rest of the globe concealed from view under their snow-white veil.

Those who have seen Mars under good telescopic 'power' will at once recognize the exact agreement between this hypothetical process and the actual appearance of the planet. All round the border of the disc there is a white light completely concealing all the features of the Martian continents and oceans. Of this peculiarity no satisfactory explanation has hitherto been advanced. I have, indeed, shown how the peculiarity would present itself if the Martian atmosphere were loaded with rounded clouds resembling our summer woolpack clouds; but it is a little difficult to believe that all over Mars such clouds as these are prevalent. Moreover, it is to be noticed that these woolpack clouds are morning and forenoon phenomena on our earth; towards noon they either vanish or become modified in shape, and as evening approaches the clouds ordinarily assume a totally different aspect, being extended in long flat sheets, the *stratus* cloud of the meteorologist. Even when rounded clouds are present in the evening sky, they are not the separate small white clouds absolutely essential for the theory formerly advanced by me; but the great heavy cloud is seen

> That rises upward always higher,
>   And onward drags a labouring breast,
>   And topples round the dreary west
> A looming bastion fringed with fire.

According to the views here suggested we have as the principal feature of Martian meteorology the melting

of the coating of hoarfrost (or of light snow, perhaps) from the ruddy soil of the planet and from the frozen surface of his oceans in the forenoon, and the precipitation of fresh snow or hoarfrost when evening is approaching. Throughout the day the air remains tolerably clear, so far as can be judged from the telescopic aspect of the planet, though there is nothing to prevent the occasional accumulation of light cirrus or snow-clouds, especially in the forenoon. I believe, in fact, that the phenomena which have commonly been regarded as due to the precipitation of rain from true nimbus clouds over Martian oceans and continents must be ascribed to the dissipation of cirrus clouds by solar heat.

But we must not fall into the mistake of supposing that because the Martian atmosphere is at so low a pressure that Martian barometers (mercurial) probably stand at only four or five inches, the atmosphere is, therefore, exceedingly shallow. Even on our earth an atmosphere producing this amount of pressure would extend many miles above the sea-level, for as a matter of fact we know that at the height of eight or nine miles, only, the atmospheric pressure is thus reduced, and even the lowest estimates assign to the atmosphere a height of fifty miles, or roughly some forty miles above the height where the pressure corresponds to five inches of the common barometer. But in the case of Mars the atmospheric pressure diminishes much more slowly with altitude than on our own earth. We have only to climb to a height of three-and-a-half miles

to find the pressure reduced to one-half (no matter what the height we start from); at seven miles it is reduced to one-fourth; and so on. But, owing to the relatively small attraction of gravity in Mars, a height of nine miles must be attained from his sea-level before the atmospheric pressure is reduced to one-half, and a height of eighteen miles before it is reduced to one-fourth, and so on. And instead of forty miles (which as we have seen is the lowest estimate of our air's height above the level where its pressure is like that of the Martian air), we find a height of fully seventy-five miles as the minimum. We may fairly assume that the Martian atmosphere extends to a height of at least 100 miles from the planet's surface.

In such an atmosphere there is ample scope for air-currents, and it is probable that owing to the tenuity of the air the winds in Mars would have a high velocity. They would not necessarily be violent winds, since the force of wind depends on the quantity of air which is in motion quite as much as on the velocity. So that we need not entertain the theory which was advanced some years since in the *Spectator* that trees in Mars must be small in consequence of the great violence of Martian hurricanes, by which all lofty trees would be destroyed. Even at a velocity of a hundred miles per hour, Martian winds would be less destructive than gales on earth blowing at the moderate rate of twenty miles per hour. But on a globe so small as that of Mars, compared at least with the earth's, swift air currents would be very effective in carrying off from the central heated

regions the moisture-laden air. In this way probably the polar snows of the planet are recruited. The polar regions must, in fact, act the part of veritable condensers, if the circulation of the Martian atmosphere is as brisk as it may well be believed to be. There must in that case be a continual gathering of fresh snows at the poles, and a continual downward motion of the glaciers thus formed, accompanied necessarily by a very active abrasion and erosion of the planet's polar regions. It seems by no means improbable, moreover, that as Mr. Mattieu Williams opines, there may be from time to time great catastrophes in these polar regions, produced by the toppling over or the rapid downward sliding of great glacial masses. For many considerations suggest that there must be an activity in the process of snow-gathering at the Martian poles altogether unlike anything known on our earth. It is noteworthy also that according to reliable observations changes have taken place in the aspect of the Martian snow-caps which imply catastrophes affecting ice-masses of enormous dimensions. Assuredly none of the changes taking place in our own polar regions could be discerned at so great a distance as separates us from Mars, save only the gradual increase and diminution of the extent of the snow-covering as winter or summer is in progress. An ice-mass as large as Spitzbergen or Nova Zembla would not be separately discernible from so great a distance, and therefore the complete destruction of such a mass by collision or downfall would be quite imperceptible at that distance, though it would be an

inconceivably stupendous terrestrial catastrophe. But masses of Martian ice quite readily discernible with good telescopes, have been found to disappear in a few hours, suggesting the most startling conceptions as to the effects which must have been produced on the comparatively small planet where these remarkable events have taken place.

The following observation, for instance, made by the late Prof. Mitchel with the fine refractor of the Cincinnati Observatory, indicates the occurrence of an event which must have been accompanied by an inconceivable uproar,—

> A wrack
> As though the heavens and earth would mingle.

'I will record,' he says, 'a singular phenomena connected with the snow-zone, which, so far as I know, has not been noticed elsewhere. On the night of July 12, 1845, the bright polar spot presented an appearance never exhibited at any preceding or succeeding observation. In the very centre of the white surface was a *dark spot*, which retained its position during several hours, and was distinctly seen by two friends who passed the night with me in the observatory. It was much darker, and better defined than any spot previously or subsequently observed here; and indeed, after an examination of more than eighty drawings, I find no notice of a dark spot ever having been seen in the bright snow-zone. *On the following evening no trace of a dark spot was to be seen, and it has never since been visible.*' Does not this observation suggest that

a great mass of ice had slipped away, leaving an intervening dark space, which in a few hours was snowed over, the gap remaining thereafter invisible? No other explanation, indeed, seems possible. But how tremendous a catastrophe to be discernible from a station some forty millions of miles away! Granting even that Mitchel used a power of 1,200 (which we find given in *Loomis's Practical Astronomy* as the highest power of the Cincinnati telescope), Mars was still viewed as from a distance of 40,000 miles with the naked eye. Let any one who has observed the aspect of an Alpine region, as seen with the naked eye from a distance of forty miles (that region being known, so that he could estimate the degree by which distance reduced even the most imposing mountain features), consider what would be the effect of removing the point of view to a distance one thousand times greater. Not merely a mountain-range, but a whole country would be invisible at such a distance. But add to these considerations the fact that the most stupendous mountain catastrophes are reduced apparently to utter insignificance at a distance of a few miles, and are altogether undiscernible at a distance of thirty or forty miles, and we shall be able to understand, though we remain utterly unable to conceive, the vastness of the catastrophe on Mars, the effects of which could be discerned when viewed as by the naked eye from a distance of 40,000 miles. One would imagine that the very frame of the small planet must have been shaken.

It does not appear to us altogether unlikely that the

varying accounts which astronomers have given respecting the polar flattening of Mars may find their true explanation in the theory we have been considering. It is certainly remarkable that eminent astronomers, like Sir W. Herschel, Arago, Dawes, Bessel, Hind, Main, and others, should have arrived at the most conflicting results on an observational matter of such extreme simplicity. We have values of the compression varying from Sir Wm. Herschel's, who made the polar diameter of the planet a full sixteenth less than the equatorial diameter, to Dawes's result that the planet is not flattened at all. Nay, some observations have even suggested that the planet is elongated at the poles. If great changes of elevation take place at the poles of Mars, owing to the rapid process of accumulation of the Martian snows, these discrepancies would be accounted for.

But whatever opinion we form on details of this sort, it appears tolerably clear that in all its leading features the planet Mars is quite unlike the earth, and unfit to be the abode of creatures resembling those which inhabit our world. Neither animal nor vegetable forms of life known to us could exist on Mars. To the creatures which thrive in our arctic regions, or near the summits of lofty mountains, the torrid zone of Mars would be altogether too bleak and dismal for existence to be possible there. Our hardiest forms of vegetable life would not live a single hour if they could be transplanted to Mars. Life, animal as well as vegetable, there may indeed be on the ruddy planet. Reasoning creatures may exist there as on the earth. But

all the conditions of life in Mars, all that tends to the comfort and well-being of Martian creatures, must differ so remarkably from what is known on earth, that to reasoning beings on Mars the idea of life on our earth must appear wild and fanciful in the extreme, if not altogether untenable.

(From the *Cornhill Magazine* for July 1873.)

METEORS—SEEDBEARING AND OTHERWISE.

ASTRONOMERS are but now beginning to recognise the full significance of those strange discoveries which have been made respecting meteors during this last four or five years. The aspect of meteoric astronomy has been completely changed by the labours of Adams, Leverrier, Schiaparelli, and a host of other inquirers; while a variety of interesting conclusions which are deducible from the recent discoveries remain as yet unnoticed, simply because so much has to be done in setting the new facts into order. Startling as was the suggestion recently thrown out by Sir W. Thomson—that meteors in long-past ages brought to our earth the seeds of life from worlds that had been shattered into fragments—I believe that even more surprising inferences will be legitimately deduced from what has been learned of late respecting meteors. Time only is needed, that, in the first place, the actual condition of the solar system, as respects these bodies, may be more satisfactorily determined; and that, in the second place, the former

condition of our system, and the condition to which it is tending, may be thence ascertained.

I do not purpose here to trace out the progress of those labours by which our present knowledge of the nature of meteors and of the part they play in the economy of the solar system has been gained. The history of those researches is full of interest, not only on account of the strangeness of the facts to which astronomers have been led, but also on account of the singular coincidences which have marked the progress of inquiry. At one time, it is a great display of shooting-stars which takes place just as astronomers required special information respecting meteoric showers; at another, a bright comet—the only comet of the 650 hitherto detected which could give certain information—appears at the very time when the information was needed; and at yet another, precisely when astronomers were inquiring about another comet supposed to have escaped detection (if it had, indeed, any real existence), they find that that very comet had been seen, its path calculated, and even its constitution determined, only a few months before. Such coincidences as these, the assiduity displayed by Adams, Leverrier, and their fellow-workers, and the singular conclusions to which their labours point, undoubtedly cause the account of the last few years of meteoric research to form one of the most interesting chapters in the history of astronomy. But the narrative of these matters has been given elsewhere, and is doubtless already familiar to most of those who will read these

pages. It would also occupy more space than can here be spared. I purpose to consider at present rather the conclusions to which recent discoveries have led, than the history of the inquiries of which those discoveries were the reward.

The first, and in some respects the most striking, feature of the new meteoric astronomy is the amazing extent of the paths on which meteors travel. There was something very startling in the conclusion to which astronomers had been already led, that meteors are bodies which, before encountering our earth, have travelled on paths comparable in extent with hers. That a tiny body—a body so light, in many instances, that a child could play with it—should for countless ages have swept around the sun on a path many millions of miles in diameter; that, in fact, such a body should have been in reality a planet, was certainly a most surprising theory. But now we know that, so far as orbital range is concerned, our earth sinks into utter insignificance beside most, if not all, of these meteoric bodies. Astronomers have only been able to determine the real paths of two meteoric systems; but these two systems afford very significant evidence respecting their fellow-systems. The members of one—the November system—travel to a distance exceeding that at which remote Uranus pursues his gloomy career; the members of the other—the August family of meteors—pass to a distance far exceeding even that of Neptune. As it is wholly unlikely that the two meteor systems first successfully dealt with are the most ex-

tended of all those which the earth encounters, the conclusion may fairly be accepted that there are meteor systems whose members travel to distances exceeding even the enormous range of the August meteors.

But there is evidence of meteoric ranges compared with which the distances just referred to are literally as nothing. It is in considering such ranges, as we shall presently see, that we touch on the question of seed-bearing meteors.

When as yet astronomers had no proof that any meteors travel on such wide paths as we have mentioned, no great reliance was placed upon the estimates of meteoric velocities as deduced from actual observation. The acknowledged difficulty of the task of observation, and some seeming discrepancies in the results, were held sufficient reasons for regarding those estimates as unreliable. For if the estimates were accepted, some very startling conclusions had to be accepted with them. Let it be remembered that a body which crosses the earth's track cannot possibly have a velocity exceeding a certain definite amount, *if* it has reached the earth's course under the sole influence of the sun's attraction. If the sun draw in meteors from surrounding space, then every one of those meteors will show by its rate of motion that it has been gathered in by the sun's might as a ruler of matter. The planets, indeed, may help the sun to some small extent; but as a rule we may leave their influence out of consideration so far as meteoric velocities are concerned. What opinion, however, are

we to form if any meteors show a rate of motion exceeding that which the sun can impart to them? If observers, having carefully watched a meteor's fiery course from two stations, deduce by calculations of a simple and convincing kind a rate of motion which is greater than that due to solar attraction, where are we to find the 'power' which has caused the meteor to travel with that extra velocity.

Now the most careful observations of meteoric movements do actually show, in several instances, a rate of motion exceeding by many miles per second that which astronomers can fairly account for. Our earth moves at the rate of eighteen miles per second, and a meteor drawn in by the sun's might from a distance exceeding even stellar distances would cross the earth's track at the rate of about twenty-six miles per second. Supposing the meteor to meet the earth full tilt, there would result but a velocity of forty-four miles per second, for the earth's attraction on the meteor would not appreciably increase its velocity. But careful observers tell us that some meteors travel through the air at the rate of sixty, or even seventy or eighty, miles per second. The extra velocity is a peculiarity too well supported by the evidence to be neglected. An explanation must undoubtedly be sought for. But whence is this explanation to be obtained?

There are other bodies in the universe which exert a mightier attraction than our sun, and are, therefore, capable of imparting greater velocities. The star Sirius, for example, must force those meteoric bodies

which circle around it to travel at a rate exceeding more than tenfold, at the very least, the velocities imparted to meteors by our sun's influence. It might seem, then, that we need only look to the larger stars—to those suns, that is, which are more massive than our own sun—for the source of these perplexing meteoric velocities. *This* would be sufficiently amazing. We should be compelled to believe in meteoric voyages, compared with which the journeys of the August and November meteors would be altogether insignificant. We should have to regard some at least of the meteors which our earth encounters as bodies which had traversed the inconceivable distances separating our solar system from the stars. And the wonder would be enhanced by the consideration that a million of years would be insufficient for the least of those tremendous voyages.

But even this explanation is insufficient. It must be accepted as true so far as it extends. Those meteors which enter the earthly atmosphere with the velocities spoken of *must* have come from extra-solar space; they *must* be visitants from the domain of other suns. This is as certain as the conclusions of astronomers respecting the past and future motions of the planets themselves. But this stupendous fact leaves the vast velocities of the meteors still unexplained. And for this simple reason:—Though Sirius and Arcturus certainly, and many other stars probably, are capable of giving to meteors travelling towards them velocities which far exceed those which our sun can impart, yet

the velocities those mighty orbs impart they also take away. Conceive for a moment the case of a meteoric body at rest in space, and about as far from Sirius as the nearest fixed star is from the sun. Sirius would draw that body towards himself, at first slowly, and afterwards more quickly, and in the course of about a hundred thousand years the body would be urging its way with inconceivable velocity amidst the planetary domain ruled over by that glorious sun. We can conceive that it would be so far disturbed on its course as not to plunge straight upon the surface of Sirius (as it would certainly do if undisturbed), but that, wheeling at its highest speed close around his mighty globe, it would pass away precisely as a comet passes away from our sun after circling closely round him. At the moment of nearest approach the body would travel at the rate of about 5,000 miles per second (at a moderate computation), and this velocity is far greater than any possessed by meteors which approach our own sun. But as the meteor swept away from Sirius, the same surpassing might which had given to the meteor this amazing velocity would continually reduce the meteor's speed. The reduction of speed in retreat would correspond exactly with the acquisition of speed in approach; and when at length the meteor had reached its original distance, although it would not be reduced strictly to rest as at first, yet the motion it would possess (due solely to the disturbing action exerted upon it while traversing the Sirian planetary scheme) would be slower than the motion of the most sluggish river. It could

never carry to other systems any appreciable portion of the velocity it had acquired while traversing the system of which Sirius is the ruling centre.

Our difficulty remains, then, still unexplained. But before searching anew for an explanation, we may note another very curious inference from what has already been shown. We have seen that meteoric bodies which travel with such enormous velocities as have been noted in some instances, must certainly have come from the domain of another sun than ours. But precisely as meteors approach our sun, and then pass away for ever, so meteors that come to us from the domains of other stars must, in many instances, have passed into those domains from the domains of yet other stars. Nor can it be regarded as likely in the nature of things that only two or three such voyages have been performed. On the contrary, it must be regarded as almost certain that, in some cases, meteors traverse inter-stellar spaces many hundreds of times, visiting each time a different stellar domain,—and perhaps even completing more than one circuit around some stars. Remembering that the least interval in which a body can pass from the domain of one star to that of another is about a million of years, we begin to recognise the wonderful antiquity of many of those bodies which have been thought fit emblems of all that is transient and perishable.

But it is when we seek for an explanation of the excess of velocity that we are led to the most startling conclusion. Let it be remembered that this excess of

velocity is now regarded by astronomers as a real fact, because the observations which had been considered as doubtful have been confirmed by what has been proved respecting certain meteoric systems. There is such a perfect accordance between the estimated and the actual speed with which the August and November meteors pass through our atmosphere that estimates of the yet higher velocities with which some meteors move cannot be looked on with suspicion. We shall presently see also that there are independent reasons for believing in these amazing velocities.

Let me premise that Dr. Mayer, in his celebrated inquiry into the part which meteors play in the economy of the universe, has set the excessive velocities of some meteors among the mysteries of the Cosmos, and that, so far as I am aware, no explanation has ever been given of the phenomenon.

Still there are two explanations which seem to be available, though one only, as I judge, is of itself sufficient to account for the peculiarity we are considering. Probably both must in any case be admitted.

The first relates to a fact which is itself among the most amazing with which astronomers have to deal. All the stars are in rapid motion, though seemingly fixed. Amidst those depths where all appears at rest motions are taking place which are so rapid that the mind is utterly unable to conceive them. Masses millions of times larger than our earth are urging their swift career through space with velocities com-

pared with which all the motions familiar to us are as absolute rest. Now, it is a well-known law of motion that each kind of movement possessed by a body takes place independently of all the others. The moon circles round the earth as if the earth were not circling round the sun. A body would circle round the moon while she circles round the earth (and with the earth around the sun), precisely as though the moon were at perfect rest. So that the motions of the bodies dependent on any star take place quite independently of the motion by which the star is sweeping onwards amid the depths of the star-system. Our earth, for instance, pursues her course round the sun as steadily as though the sun were at rest, instead of being in rapid motion with all his *cortège* of planets. And the power which a star has of communicating velocity to an approaching dependent body, and of withdrawing velocity from a receding body, has no reference to the motion which the body shares with the star. Take the case of Sirius, for instance. In what I said of him above I regarded him as at rest; and I stated, justly, that he could communicate to a body approaching him from a state of rest an enormous velocity, the whole of which he would withdraw during the recession of the body. But Sirius is, in reality, travelling with great velocity amid the star-depths; and if we conceive the case of a meteoric body circling close around Sirius with the enormous velocity already referred to, we must remember further that that body shares also with Sirius the great

velocity wherewith the star is being carried through space. The first velocity Sirius has himself communicated, and he not only can, but will, withdraw it wholly from the meteor; but the other velocity he has not imparted, and neither can he withdraw it. The meteor will pass away, and will be reduced to all but rest with respect to Sirius,—that is, to a condition in which it neither approaches nor recedes from the star; but this very state of rest with respect to Sirius implies an enormous velocity with respect to space. Precisely as a body at rest on Sirius, or within his mass, is being carried at the rate of some thirty miles per second through space, so would our meteor possess this enormous real velocity, though reduced to all but absolute rest with respect to Sirius.

Now, bodies passing from the domain of one star to that of another must carry with them this balance of motion which their late ruler has been unable to touch. The effect will be different according to the manner in which they enter the domain of their new ruler; but it may happen in many cases that they will appear to move with the whole of this velocity as an excess of motion over and above that due to the sway of the star ruling them for the time being.

At first sight, it seems as though we had here a sufficient explanation of the peculiarity we are considering. It will presently be shown that some difficulties still remain. But before passing on, let us consider the strange explanation we have been dealing with.

It is, in the first place, a surprising circumstance that the stars should travel so swiftly as they do, amid the depths of space. We do not here speak of this circumstance as surprising merely in the sense in which so many astronomical facts are surprising. It is startling to consider that Sirius is more than a thousand times more massive than our sun, or that the sun is more than a million times larger than the earth on which we live. But there is nothing in these or similar facts, which is not in accordance with our ideas respecting the constitution of the universe. The rapid motions of the stars, however, present a source of grave perplexity, in the circumstance that motion is a measure of force, and we cannot understand what the force can be which has produced these motions. The mutual attractions of the stars are utterly unequal to the generation of velocities so enormous. The stars which are the next neighbours of any given star are those which tend most effectually to excite motion in that star; and their attractions counteract each other because acting in different directions. But supposing all these stars removed to one side of the first, so as to combine their attractions upon it, even *then*, at the enormous real distances separating the stars from each other, the resulting motions would not be comparable with those which actually exist. Thus we have, in the motions of the fixed stars, the evidence of a mighty force other (it would seem) than gravity, and perhaps acting according to other laws.

Now, if the assumed explanation of the rapid

motions of meteors be correct, these bodies bring before us, in the most direct possible manner, the effects of this mighty force. They penetrate the atmosphere of our earth with velocities generated either by attracting bodies—non-luminous stars for instance—other than those we are cognizant of, or else by forms of force distinct from the attraction of gravity. Here, then, we have a conception respecting these bodies which is even more startling than the conception that they may be fragments of an exploded world, or that they may bear with them the germs of life. It is true that we know of no instance in which a world has exploded, for astronomers no longer imagine that the asteroids are fragments of a world which once travelled between the paths of Mars and Jupiter; nor is it very easy to conceive how the germs of living things can be preserved under the conditions to which meteors are subject. But volcanic action shows us at least how worlds might be supposed to explode; for we commonly compare a volcano to a safety-valve, and the purpose of a safety-valve is to prevent explosion. And again, the idea of the conveyance of the germs of life from place to place is one with which we are sufficiently familiar. But in the motions of the meteors we have evidence either of the existence of bodies differing from all with which we are acquainted,—more massive than the suns, but as opaque as the planets,—or else of the action of a force mightier than the force of gravity.

While we may admit, however, that in many in-

stances the great velocities of meteoric bodies may be due to the proper motions of those stars from whose domains the meteors have reached our earth, yet it is difficult to regard this explanation as altogether sufficient. In the first place, there are few stars whose motions are large enough to avail for our purpose. Sirius has a rate of motion altogether exceptional; and it is probable that the average rate of stellar motion does not exceed four or five miles per second. And again, only a small proportion of the meteoric bodies coming from the domain of one star to that of another would show traces of the kind of motion we have been considering. Certainly very few would show an excess of velocity, corresponding to the rate of seventy or eighty miles per second, with which meteors have been observed to traverse our atmosphere.

The second explanation of which I have spoken seems required to interpret what still remains unaccounted for. This explanation is so startling that at first sight few would be disposed to admit it as even a possibility. However, when theories so surprising as Sir W. Thomson's hypothesis of seedbearing meteorites are submitted to the gravest scientific assemblies, I need not fear to present even so startling a theory as the one I am about to deal with, more especially as I shall be able to exhibit certain very singular evidences in its favour.

The second explanation is simply this: that a large portion of our meteoric visitants have been expelled or erupted from the stars—including our own sun.

The most obvious objection to this hypothesis resides in the fact that what appears an utterly incredible velocity must be communicated to the expelled matter in order to render the explanation available. It is necessary that the stellar volcanoes should propel meteoric matter from their interior with a velocity sufficient to free the missiles for ever thereafter from the control of their parent star. Now, to take the case of our own sun, any matter shot forth from his interior at a rate of less than 380 miles per second would return to him again under the influence of his far-reaching attraction. It would, if undisturbed by planetary attraction, return (after a long excursion) in such sort as to strike his surface as squarely as it had left that surface. But even taking into account all disturbing forces, it would still return to the sun. A velocity exceeding that just named would free the erupted matter from the sun's influence,—to this extent at least, that though the sun would continually retard the motion of the receding matter, he would never be able to destroy that motion or change it into a motion of approach. But it seems incredible that any forces residing in the sun would be competent to propel matter from his globe at a rate so enormous.

And yet the evidence obtained during the past few years respecting the motions of the solar prominences seems to show that a velocity fully as great as that which I have spoken of may be imparted to matter expelled from the sun's substance. It is generally admitted that the prominences are due to some eruptive,

or at least repulsive force exerted by the sun. They have been seen to form like jets from a fountain, rushing forth with incredible velocity till they have attained their greatest height, and then falling back, more or less rapidly, towards the sun's surface. Now estimates have been formed respecting their velocity by direct observation, the powers of the spectroscope availing for this purpose. But we have in the height to which the prominences attain, the most satisfactory evidence of the velocity with which the gas comprising them has been propelled from the sun's interior. Assuming only that the prominences are formed of projected matter, we can as certainly determine the rate of propulsion as we can determine at what rate a cannon-ball must be propelled vertically upwards from the earth's surface to reach a given height. At least, we can determine the lowest velocity which would suffice,—supposing we see the full height of the prominences, and nothing happens to check the upward motion of their gaseous substance. But as surely as we know that a cannon-ball must be fired at a much higher velocity to attain a given height through the air than would suffice if it could be fired in a vacuum, so also we can infer that whatever velocity we deduce for the prominence matter, regarding it as projected through vacant space, must fall very far short of the real velocity. May we not even go farther, and consider rather the velocity with which the smoke leaves the mouth of the cannon as compared with that necessary to carry a projectile only to the height reached by

the smoke? If we accept this view, and certainly the constitution of the prominences favours the supposition, we should decide that there can be scarcely any comparison between the velocity with which the matter of the prominences is projected from the sun's interior and that which would carry a projectile in a vacuum to the observed height of the prominences.

Now the largest prominence yet seen had the enormous height of 160,000 miles; and a projectile from the sun would require a velocity of 200 miles per second at starting to attain, even through vacant space, to this vast height. It will scarcely be thought too daring to assert that the matter of this prominence must have had at least twice this velocity at starting, under the actual circumstances of resistance to which its motion was exposed. Here, then, we have evidence of a propelling force in the sun fully equal to the discharge of meteoric matter in such sort as never to return either actually to his globe or on an orbital path close by him. If the same discharge which propelled the gaseous substance of the great prominence to a height of 160,000 miles carried some denser substance along with it (which seems not only credible, but exceedingly likely), and if that substance by virtue of its density passed with much less loss of velocity through the solar atmosphere (as a cannon-ball retains much more of its velocity than the gases propelled along with it), then, unquestionably, the sun rejected that matter *for ever* from his substance on the day that the great prominence was formed. The gas of the promi-

nence was checked in its outward course by the resisting solar atmosphere, the denser matter only by the sun's attractive force; and this force, inconceivably mighty though it is, could only deprive the departing matter of a portion of its velocity. A portion amounting to about 125 miles per second would still remain, and would carry the erupted matter away through space until it entered the domain of some other sun.

It will be observed that, startling though the theory may seem, there is nothing forced about any of the suppositions on which it is based. The theory that the solar prominences are phenomena of eruption is regarded by the leading observers of these objects as highly probable, if not certain. The aspect of these strange formations shows that they are flung through a resisting medium; and therefore it is certain that they must be projected much more swiftly than we should infer by merely regarding them as projectiles flung through a vacuum. It seems highly probable that, as in the case of terrestrial volcanoes and geysers, denser matter is flung forth along with the gases of the prominences. And it is certain that such matter, like the ball from a cannon, or stones and cinders from a volcano, would be much less affected by the resistance of the atmosphere than the lighter gaseous matter projected along with it. Admitting these four postulates, of which two are highly probable, and two certainly just, it follows, as an inevitable conclusion, that the sun rejects matter from his substance,—such rejection being

final, owing to the enormous velocity imparted to the erupted matter. And if the sun thus rejects matter, so also do those other suns, the stars. Let us add to this reasoning two facts which have been regarded as severally sufficient to establish the strange theory that many meteors, if not most or even all, have been expelled from the interior of the suns which people space.

One of the most eminent microscopists living, and perhaps the most eminent of all who have applied the microscope to the study of rock-substances—Sorby, of Sheffield—has arrived at the conclusion that the structure of meteorites 'cannot be explained in a satisfactory manner, except by supposing that their constituents were originally in the state of vapour, as they now exist in the atmosphere of the sun.'

Again, the late Professor Graham, one of the most eminent chemists of our time, was led to a similar conclusion by the chemical analysis of a meteor. He had found that the iron of the Lenarto meteor contains much more hydrogen ('occluded' in its substance) than can be forced into the substance of malleable iron. 'It has been found difficult,' he says, 'to impregnate malleable iron with more than its own volume of hydrogen, under the pressure of our atmosphere. Now the meteoric iron (this Lenarto iron is remarkably pure and malleable) gave up about three times that amount, without being fully exhausted. The inference is, that *the meteorite was extruded from a dense atmosphere of hydrogen gas*, for which we must look beyond the

light cometary matter floating about within the limits of our solar system. . . . Hydrogen has been recognised in the spectrum analysis of the light of the fixed stars by Messrs. Huggins and Miller. The same gas constitutes, according to the wide researches of Father Secchi, the principal elements of a numerous class of stars, of which Alpha Lyræ is the type. The iron of Lenarto has, no doubt, come from such an atmosphere in which hydrogen greatly prevailed. This meteorite may be looked upon *as holding imprisoned within it and bearing to us the hydrogen of the stars.*'

I do not indeed suppose that all meteors have had an origin of this sort. It is almost impossible to examine the facts which have been made known respecting meteors without arriving at the conclusion that no inconsiderable proportion of these bodies have not as yet formed part of any of the more massive orbs which are spread throughout the realms of space. It would seem as though two processes were simultaneously at work. On the one hand, there is an aggregating process, by which meteoric matter is brought to the surface of orbs, such as the sun and his fellow suns, our earth and other planets, the moon and other orbs which, like her, circle around the members of our own and other solar systems. On the other, there seems to be a process of rejection by which meteoric matter is continually being projected from the substance of the sun and the countless millions of other suns which constitute our galactic system.

I may remark, in passing, that it is not as yet

clear whether comets, which are undoubtedly associated with meteors in some unexplained way, are to be regarded as composed of matter which has never yet belonged to the substance of a sun, or as composed of sun-rejected matter. But the paths followed by some comets would lead to the conclusion that these comets at least have been projected with considerable velocity from the interior of stars. It is well known that some of the comets which have appeared in our skies have been found to traverse paths so shaped that the comet cannot possibly return to our sun's neighbourhood. When a comet has a path of this sort, we see that it does not belong to our sun's domain, for it is free, after its visit, to retire into the depths of space; nor can the comet have belonged to the domain of the sun it last visited, for otherwise it would not have been free to visit our sun's realm: and tracing back the comet's course through as many visits to different star domains as fancy may suggest, we yet never find that it could have belonged to the domain of any star. The only conceivable explanation of its first appearance on the stellar scene seems to be that which regards it as ejected bodily from some orb among those which shine amid the depths surrounding us. It may seem fanciful to recognise the action of the same sort of repulsive force which first ejected the comet, in the repulsive effect undoubtedly exercised on the matter composing the tails of comets which approach our sun. Yet after all, this repulsive effect, and the enormous velocity of motion which it is capable of producing (as Sir John

Herschel has shown) may afford perhaps the most satisfactory solution of the difficulties we have been considering.

The seed-bearing meteors of Sir W. Thomson, if their existence be admitted, must be regarded as holding an intermediate position between the two classes of meteors above referred to. They have neither been for all ages unattached wanderers through space, nor certainly have they been rejected from the fiery interior of a sun such as ours. In fact, Sir W. Thomson tells us very definitely what they are,—they are the fragments of worlds which have been destroyed by collision. It is desirable to present Sir W. Thomson's reasoning in his own words (according to the fullest reports), because full justice has not always been done to him when his startling hypothesis has been described or summarised. The theory is amazing enough even as he presents it; but it is rendered utterly absurd by some of the modifications which it has received in the mouths of exponents.

Let us first consider how the theory was suggested. The questions which have recently been raised respecting the origin of life could scarcely pass unnoticed in a review of the scientific work of the year 1872. Accordingly, Sir W. Thomson, as President of the British Association, seemed invited to their discussion. 'How did life originate,' he asks, 'upon the earth? Tracing the physical history of the earth backwards, we are brought to a red-hot melted globe on which no life could exist. Hence, when the earth

was first fit for life, there was no living thing on it. There were rocks, water, air all round, warmed and illuminated by a brilliant sun, ready to become a garden. Did grass, and trees, and flowers spring into existence, in all the fulness of a ripe beauty, by a fiat of Creative power? or did vegetation, growing up from seed sown, spread and multiply over the whole earth? Science is bound, by the everlasting law of honour, to face fearlessly every problem which can fairly be presented to it. If a probable solution, consistent with the ordinary course of nature, can be found, we must not invoke an abnormal act of Creative power.'

He then proceeds to consider under what circumstances regions which in some respects resemble, or may be supposed to resemble, the lately cooled earth, become under our eyes the abode of abundant life. 'When a lava stream flows down the side of Vesuvius or Etna it quickly cools and becomes solid; and after a few weeks or years it teems with vegetable and animal life, which—for it—originated by the transport of seed and ova, and by the migration of individual living creatures. When a volcanic island springs up from the sea, and after a few years is found clothed with vegetation, we do not hesitate to assume that seed has been wafted to it through the air, or floated to it on (natural) rafts.'

'Is it not possible,' he proceeds to ask, 'and, if possible, is it not probable, that the beginning of vegetable life on the earth is to be similarly explained? Every year thousands, probably millions, of fragments

of solid matter fall on the earth. Whence come these fragments? What is the previous history of any one of them? Was it created in the beginning of time an amorphous mass? This idea is so unacceptable that, tacitly or explicitly, all men discard it. It is often assumed that all, *and it is certain that some*, meteoric stones are fragments which had been broken off from greater masses and launched free into space. It is as sure that collisions must occur between great masses moving through space, as it is that ships steered without intelligence directed to prevent collision could not cross and recross the Atlantic for thousands of years with immunity from collisions. When two great masses come into collision in space, it is certain that a large part of each is melted; but it seems also quite certain that, in many cases, a large quantity of *débris* must be shot forth in all directions, much of which may have experienced no greater violence than individual pieces of rocks experience in a landslip or in blasting by gunpowder. Should the time *when this earth comes into collision with another body*, comparable in dimensions to itself, be when it is still clothed as at present with vegetation, many great and small fragments, carrying seed and living plants and animals, would, undoubtedly, be scattered through space. Hence, and because we all confidently believe that there are at present, and have been from time immemorial, many worlds of life besides our own, we must regard it as *probable in the highest degree* that there are countless seed-bearing meteoric stones

moving about through space. If at the present instant no life existed upon this earth, one such stone falling upon it might, by what we blindly call natural causes, lead to its becoming covered with vegetation. I am fully conscious,' adds the learned mathematician, in conclusion, 'that many scientific objections can be urged against this hypothesis; but I believe them to be all answerable,—the theory that life originated on this earth through moss-grown fragments from the ruins of another world may seem wild and visionary; all I maintain is, that it is not unscientific.'

Before considering the statement as to the movements of masses through space, on which, as on certainties, Sir W. Thomson has based his hypothesis, it may be well to touch briefly on a few incidental considerations. In the first place, it will be noticed that the hypothesis accepts to the full the principle of development as respects life on the earth. For it professes only to explain how the earth may have become covered with vegetation, that vegetation being presumably developed from a few primal forms, introduced by meteoric agency. The lower forms of animal life would then be developed from certain forms of vegetable life, and thence higher forms of animal life, and (on our earth at least) man as the highest form. It is, again, to be noticed that the theory does not profess to explain the origin of life generally, but the origin of life upon our earth. Of the two orbs whose collision led to the scattering of seed-bearing meteorites for our earth's benefit, one, at least, must

have been already the abode of life. The difficulty of the problem discussed by Biogenists and Abiogenists is removed but a step, and remains untouched for one who is ready, with Sir W. Thomson, to adopt as 'an article of scientific faith, true through all space and through all time, the theory that life proceeds from life, and from nothing but life.' Nor must the fact remain unnoticed that meteors have never been found which either contain or show traces of having once contained the germs of life. It might be expected that if a globe so vast as our earth could be peopled with all the forms of vegetable and animal life now existing on its surface, through the agency of meteoric stones, some signs of the seed-bearing character of meteors would be recognised by microscopists. Yet neither the solid bulk of meteoric stones, nor the light meteoric dust which seems to be at all times sinking through the air, has revealed, under the closest microscopic scrutiny, the slightest trace which could be regarded as confirmatory of Sir W. Thomson's hypothesis.

But the hypothesis is so clearly expressed as to leave us in no doubt of the nature of these probabilities, possibilities, and certainties, in which its author believes. We need not pass backwards to the former history of our own earth, but may proceed to discuss its future fate, as predicted according to this hypothesis. Our earth is certainly to 'come into collision with another body comparable in dimensions with itself,' and then (if only the earth is in its present condition, as respects

the existence of life upon its surface) 'many great and small fragments, carrying seed, and living plants and animals, will undoubtedly be scattered through space.'

Now, I venture directly to deny this proposition that the earth will one day *certainly* come into collision with another. It does not seem to me certain even that, amid all the orbs which people the infinity of space, there are two (tenanted by living creatures) which will, even in the infinity of future time, be destroyed by collision. It is absolutely certain that all the primary orbs of our solar system are safe from *mutual* collision or dangerous approach, either now or at any future time; so also the moons of Jupiter are safe from collision or dangerous approach; the moons of Saturn equally safe; and so also the moons of Uranus. Now, so far as analogy can guide us, the suns which form our galaxy must be regarded as equally safe from mutual collision or dangerous approach. Simply by virtue of their motions under the action of gravity, they must be held—judging from analogy—to traverse paths as free from mutual intersection as the paths of the primary orbs of the solar system. If this is so, the worlds over which any sun bears sway are as safe from all risk of collisions with worlds belonging to the domain of other suns as are the moons of Jupiter from risk of collision with the moons of Saturn. It may well be, or rather it is highly probable, so far as all known analogies are concerned, that every risk of collision between worlds belonging to different systems is thus removed. But even if it were not so, if there really is

a possibility that some worlds may come into collision and be destroyed, it assuredly cannot be predicated as a certainty respecting any given world—our own for example—that it will be destroyed by collision at some future date, however distant. Amidst the star-depths, with their uncounted millions of suns—each, perhaps, the centre of a scheme no less important or even more important than our solâr system—where does Sir W. Thomson find the suns which, by their dangerous proximity to each other, seem to countenance his hypothesis? Not surely among the double, or multiple stars; for whatever collisions may occur among their dependent orbs must be regarded as mere family contests not competent to affect other systems. *Where then?* Astronomy answers confidently that there is no evidence of the sort.

But let us grant, for a moment, that our earth has come into collision with another world, and that many great and small fragments carrying seed, and living plants and animals, are scattered through space by the collision, and let us endeavour to ascertain the conditions under which one of these fragments may carry the germs of life to some distant world. It need scarcely be said that the living plants and animals would quickly perish, so that we have only to consider the possibilities relating to the vegetable germs. Now we have no means of determining exactly how long a vegetable germ may retain potential life. Corn-seeds from the Pyramids have germinated, under suitable conditions, here in England, and in our own age; and it is con-

ceivable that the thousands of years which have elapsed in this instance might become millions of years without the vitality of the seeds being affected. Furthermore, it is conceivable that seeds may bear strongly-marked vicissitudes of cold and heat without the destruction of the vital principle contained in them. Yet, when we learn that the fragments of our destroyed earth would be millions of years amid the cold of space (a cold far below the freezing point), before they approached the domain of another star—even though they made for the nearest star in the heavens—we certainly are not led to entertain a very strong conviction that they would germinate in the first world they chanced to encounter there, or that they would become the means 'by what we blindly call natural causes, of its becoming covered with vegetation.' But we have further to consider that if our earth were scattered into a million fragments, the chances would be many millions of millions to one against any one of these fragments following a course which would lead it to collision with some world, after but *one* interstellar voyage. It is altogether more probable that every one of the fragments would visit in succession many stars, occupying millions of years in flitting from one to the next, sweeping so closely around some as to be melted, or even vaporized, and subject during the intermediate millions of years to a degree of cold of which we can form no adequate conception. Is it over-daring to assert that no germs would retain the vital principle after such a series of voyages?

It seems to me that astronomers are not free to admit the existence of a class of meteors intermediate to those already considered. There are meteors which bear strongly-marked traces of having been ejected from other suns than ours; and, on the other hand, there are meteors which would seem not to have as yet formed part of any large orb in space. But we have abundant reasons for questioning whether any meteors are fragments of worlds which have once been the abode of life; while assuredly we seem entitled to reject decisively the theory that such fragments could bear the seeds of life to other worlds. The great mystery of the origin of life upon our own earth has not yet been solved, nor has a path towards its solution been discovered; and even if the strange hypothesis we have been considering had appeared admissible, the mystery of the origin of life in the universe would have remained as inscrutable as ever. The great problem which is at present engaging the attention of biologists—the question whether all the forms of life now existing on the earth have been developed from a few simple forms, or even one—would in fact be replaced by this infinitely more stupendous problem, the question whether all the forms of life existing in all the worlds throughout space have had their origin in some primal form existing at an infinitely remote epoch. And one circumstance, which to some extent gives countenance to the hypothesis of development, even to the mind of those who desire to form the noblest conceptions of the nature of the Deity, is wanting from this amazing ex-

tension or perversion of the hypothesis. Development, rightly understood, implies the perfect working of laws assigned to the universe in the beginning by the Creator; but, according to this new doctrine of development, life passes from world to world in a series of catastrophes. It was rightly objected by Leibnitz, that the views of some religious men in his day implied that the machine of the universe required continual winding-up; but this is little to the teaching of the new hypothesis, according to which the progress of the universe is only secured by repeated collisions. Others, again, have urged that Nature, so far as we can comprehend her acting, seems 'in filling a wine-glass, to upset a gallon;' but it was left to this new theory to show that she must destroy two worlds in order to plant a few moss-seeds in a new one.

[NOTE.—Since the above pages were written, Professor Young has witnessed a solar eruption in which glowing hydrogen passed from a height of 100,000 to a height of 200,000 miles in ten minutes. The last-named height would imply ejection at the rate of 213 miles per second; but I have found, after a careful calculation, that matter ejected at this rate would occupy twenty-six minutes in traversing the observed distance. Hence the velocity of ejection *must* have enormously exceeded 213 miles per second, and atmospheric resistance *must* have acted, depriving the upflung hydrogen of a portion of its excessive velocity, and limiting its range of flight. More condensed matter, flung up along with the hydrogen, would retain a much larger share of the original velocity, which probably exceeded 500 miles per second. Such matter passed for ever from the sun's domain.]

(From the *Cornhill Magazine* for November 1871.)

A RECENT STAR-SHOWER, AND STAR-SHOWERS GENERALLY.

On November 27, 1872, several days after astronomers had given up all hopes of recognizing any conspicuous traces of the great November meteor system, there occurred a remarkable display of shooting-stars. Meteors fell for about five hours at an average rate of nearly two thousand per hour. In brilliancy they were not to be compared with those which fell during the memorable display of November, 1866; but they were fully as numerous.

It is not to be wondered at, perhaps, that after so much has been said and written about the November meteors, many of the observers of the recent display should have supposed that it belonged to that now celebrated system or stream. Were this the case, although the phenomenon would not be without interest, yet I should scarcely care to discuss it in these pages. For, to say truth, the subject of the November shooting-stars has become somewhat well-worn; and moreover, there is scarcely a feature of that system which may not be found fully described in extant treatises on astronomy. But, as a matter of fact, not only was the recent display altogether unconnected with the Leonides (as the November meteors are called), but there are circumstances which give it a peculiar interest and significance. Carefully considered, it will be found to throw a new light on the whole subject of meteoric astronomy, and especially on that

very remarkable relation which associates meteors with comets.

Since it chances, also, that a new and somewhat startling theory of the origin of certain meteor systems has recently been propounded, and is apparently supported by evidence of considerable weight, I think that this is a favourable opportunity for again touching on a subject which in November, 1871, I discussed (see preceding essay) at some length. In the paper referred to,—entitled Meteors—Seed-bearing and otherwise,'—I showed reasons for believing that some meteoric masses which fall on our earth have been expelled either from the sun or from one or other of his fellow-suns, the stars. I shall now show how similar evidence must (it would seem) be interpreted in the case of meteor-systems which certainly have not come from the sun, while a stellar origin is almost as certainly out of the question. The meteoric system which produced the falling-stars seen on November 27, 1872, belongs to this category.

But it may be asked in the first place how we can be sure that those shooting-stars were not Leonides. The answer is not far to seek.

In the first place every shower of falling stars comes as certainly from a definite direction as a shower of rain falling when a strong and steady wind is blowing; and apart from all question of date, a star-shower falling in one direction cannot well be mistaken for one falling in another direction. Now the contrast which exists in this respect between the so-called

November meteors—that is, the Leonides—and those which fell on November 27 last, was of the most decided character. The Leonides, regarded as a shower, meet the earth almost full front; the shower of last November fell from behind. Here I refer to the earth's motion in her orbit. As she circles round the sun she of course always directs her motion towards some point upon that circle in the heavens which marks the sun's seeming yearly journey round the earth; and in November her course is directed towards the constellation of the Lion. It is from out that constellation—so far as appearances are concerned—that the Leonides travel to meet her. Were it not for a slight fall from the north, it might be said that they meet her full front; and their course may be compared to that of a shower borne by a fierce wind, and meeting a person who travels so as directly to face the wind. But the shower of November 27 came from a region of the heavens lying somewhat north of the constellation Aries, which, as every one knows, is a long way from the Lion: the Ram is indeed only separated by the Fishes from the Water-bearer; and on November 27 the earth is travelling directly away from a point in the Water-bearer on the side towards the Fishes. So that the shower fell from behind. It came downwards, also, or from the north, with a much greater slant than in the case of the Leonides; and it fell from the outer side of the earth's track—that is, from the side away from the sun.*

* If a somewhat commonplace illustration (but the aptest which occurs to me) be permitted, I may compare the Leonides to a shower

There can be no possibility, then, that the shower of last November was in any way connected with the Leonides.

But another question will at once suggest itself. Astronomers have lately been in the habit of predicting star-showers, and on the whole not without success. How was it that the late shower had not been in any way announced?

To this it must be replied in the first place that meteoric astronomy is a science of quite recent birth. A very few years ago astronomers were far from feeling assured that meteors are astronomical phenomena at all. Very few facts were known which could enable astronomers to make predictions even respecting one or two of the more remarkable meteor systems; while it was clearly recognised that a far greater number were unclassified. These meteors — *sporadic*, as Humboldt called them—might belong to systems capable of giving rise on occasion to great meteoric displays, precisely as the Leonides for a score of years in succession may show only a few stragglers, and then during several years produce remarkable showers. *But* astronomers could only recognise the *possibility* of such displays in the case of nearly all those meteor streams which were known only by their stragglers. In the great majority of instances there is absolutely

of missiles thrown at a rider in a circus from the front seat in the lower tier of boxes so as to strike him in the face, while the shower of last November may be compared to a shower thrown from a back seat of a higher tier so as to strike him from above and from the outside, but also slightly from the rear.

no way whatever of forming the vaguest idea as to the possible occurrence of star-showers belonging to any given system. There is scarcely a night in the year when a display of meteors may not occur—a display belonging to a recognised meteor stream, but to one whose period has not been determined.

The very fact that astronomers recognise an association between meteors and comets shows that a long interval must elapse before the periods of most of the meteor systems can be determined. For we see that the November meteors exhibit their great displays at intervals of about thirty-three years, this being the period of the comet they belong to; and the August meteors have probably an interval of about 150 years, corresponding to the period of the comet of 1862; but many of the comets observed by astronomers have periods measured not by decades but by centuries, and even by thousands of years.

It however chances that, in a quiet sort of way, astronomers had discussed the possible occurrence of a star-shower during the last week in November; and as we shall presently see, the shower which actually occurred was the one thus looked for. The circumstances are somewhat remarkable.

There is—or, perhaps we should rather say, there was—a celebrated comet called Gambart's by the French, but by all other astronomers named after the German astronomer Biela. This comet was first fairly discovered in February, 1826, though it had been seen in the years 1772 and 1805. It was found to revolve

round the sun in about $6\frac{3}{4}$ years, and on a path approaching very closely indeed to the earth's. Indeed, great alarm was experienced when the return of the comet was expected in 1832; for it was shown by the calculations of astronomers that the head of the comet would actually envelope the part of the earth's orbit which she crosses on November 30. This, in fact, happened; but since it happened (as predicted) on the 29th of October, 1832, when the earth was a month's journey from that part of her orbit, of course there was no collision. Twice more the comet circled on its wide path, extending far beyond the orbit of Jupiter; and again in 1846 it was in our neighbourhood, and under the careful telescopic scrutiny of astronomers. It was then that, almost under their eyes, the comet behaved in a most remarkable manner. Up to the 13th of January it had presented its usual aspect, a round hairy-looking head enclosing a brighter nucleus, and a narrow, straight, and rather short tail; but on that day, to the astonishment of the telescopist who first recognised the change, the comet was found to be double. There, in the place of the single comet of the day before, were 'two distinct comets,' says Sir John Herschel, 'each with a head, coma, and a little nucleus of its own;' and each with a tail also, if the pictures by the German observers are to be trusted. 'What domestic trouble caused the secession,' proceeds Herschel, 'it is impossible to conjecture, but the two receded farther and farther from each other up to a certain moderate distance, with some degree of mutual

communication and a very odd interchange of light—one day one head being brighter and another the other—till they seem to have agreed finally to part company. The oddest part of the story, however, is yet to come. The year 1852 brought round the time for their reappearance, and behold! there they both were, at about the same distance from each other, and both visible in one telescope.'

In 1859, when the comet next came near the earth, circumstances were such that it was useless to look for it. In fact, it was so situated as to be lost in the sun's light. But in 1866 the double comet should have been well seen. In the confident expectation that both comets would appear, astronomers carefully searched the track which had been calculated for these bodies, but without success. In some as yet unexplained way both the comets had been either dissipated or destroyed.

Now, during the autumn of 1872, another period of the comet's revolution was completed, and again a search was instituted. Mr. Hind calculated the path of the chief comet of the pair on three different suppositions as to the date of nearest approach; more care was expended in the search than in 1866 (when the non-appearance of the comet took astronomers by surprise); but all attempts to rediscover the comet proved ineffectual, and it is now generally believed among astronomers that Biela's comet will never be seen again *as a comet.*

I do not discuss here the probable manner or

cause of the comet's dissipation, because for our present purpose we only require to consider those parts of the comet's history which are related to the recent meteoric display. The association between the star-shower and the vanished comet must now be indicated.

We have seen that the comet's path crosses close by the earth's. Consequently, the association between comets and meteors now recognised by astronomers leads us to expect that the earth, in crossing the comet's track, would be saluted by meteors. What we have learned from the November meteors teaches us further to expect that the most remarkable display of these Bielan meteors would occur when the earth crossed the comet's track soon after the comet had 'gone that way.' So that since the comet passed early last autumn, it was to be expected that the earth's passage of the comet's track late in the autumn months would be attended by a meteoric display.

Now, we have seen that in 1832 the earth's destruction was expected because the comet crossed the part of the earth's orbit which she traverses on November 30. So that if matters had remained unaltered a display of Bielan meteors was to be expected on the night of November 30. But it was known that the place of passage, owing to perturbations of the comet, is shifting backwards along the earth's track, and, as a matter of fact, November 27, the very day of the recent star-shower, would correspond with the time of the earth's crossing the comet's track. For Dr. Weiss, a German astronomer, calculated that in 1858 the date of

passage would be November 28; and comparing this with the fact that in 1830 the date was November 30, we see that the date for the present year would be November 27.*

Next, as to the direction in which Bielan meteors would salute the earth. The comet travels in the same direction round the sun as the earth, and, moving more swiftly than the earth when in her neighbourhood, can manifestly only reach her from behind. The comet also crosses her track from the outside, travelling on a path slightly inclined downwards, or from the north. All this agrees with what was stated above, as to the course of the meteors seen last November. But the agreement is very close indeed when details are considered. We can tell the very part of the heavens whence the comet would seem to come if at any time it actually overtook the earth; and this part of the heavens (making the calculation for the year 1872) would be close to the feet of Andromeda. Now, the best accounts of the recent meteoric display describe the shower as having its 'radiant point' in that very region of the heavens. By the best accounts, I do not, of course, mean those which agree best with the theory that the meteors

* It would not be desirable to enter here into the recondite considerations affecting the estimated position of the comet's path. What is said above suffices to show, in a general way, that November 27 is about the right date at present; but the real shifting of the date takes place in a very complicated fashion, the mere description of which would be quite unsuited for these pages, while the explanation could only be understood by the mathematician.

belonged to Biela's comet, but those which came from the most practised observers. To show that I am not exaggerating the value of the evidence on this point, I quote the account received from Professor Grant, of the Glasgow Observatory. Professor Grant is not only known as a most skilful astronomer and mathematician, but he has also paid special attention to meteoric phenomena. I give the whole account, partly because of its general interest, and partly because it gives weight to the determination of the radiant point. 'At 5.35,' says the account, 'when a series of observations were commenced by Professor Grant and his assistants at the Observatory, only forty meteors were seen to fall within five minutes. The number gradually increased, the maximum being attained at a quarter past eight, when 366 were counted by one observer in five minutes. As the region of observation of any one person was necessarily limited, we may assume that the whole number actually visible in the heavens was much greater, and that it would not be less than 1,600 within the time specified. From 8.15 onwards the meteoric shower gradually lessened, until at 10.10 only fifty shooting-stars were counted in five minutes. The radiant point was a little above the star Gamma Andromedæ. At times three or four meteors darted at once across the sky, like a shower of serpents, some of them leaving behind masses of light of a pale red colour.' Again, Mr. E. J. Lowe, of the Highfield House Observatory, Nottingham, telegraphed as follows on Wednesday night: 'From ten minutes to six to the

present time (7.30) a very great shower of meteors has fallen. They still continue. The radiant is in Andromeda.'

It thus appears certain that the display of November 27 was a shower of Bielan meteors. But the reader may desire to have some evidence showing that this is not an after-thought, but in accordance with ideas expressed before the display took place. It may be well, therefore, to mention that, in the Monthly Notices of the Astronomical Society published on October 24, 1872, a month before the star-shower, there appeared a list of 132 meteor streams, amongst which is one (No. 120), whose date is set at November 30 by the Italian observers, Schiaparelli and Zezioli, while our English meteor-students set it down for November 25; and not only is the radiant of this shower set in Andromeda, but the remark is appended that the shower is 'supposed by d'Arrest and Weiss to be connected with Biela's comet.' Then, in the same number, but in another article, Professor Herschel invites astronomers to be on the watch for a display of Bielan meteors, mentioning that 'the date of the earth's passage through the comet's orbit now falls in the end of November.'

But the actual appearance of the shower, so soon after Biela's comet had passed, combined with the perfect agreement between the movements of the meteors and the position of the comet's path, must be regarded as rendering certain that which before had been but highly probable. Those who witnessed the

display of November 27 may be perfectly assured that they were then watching the fall of bodies associated with one of the most interesting of all the comets ever studied by astronomers. The collision so dreaded in 1832 was in 1872 actually in progress; the process of dissipation commenced in January 1846, was illustrated and, perhaps carried to a further stage last November; and, lastly, the disappearance of Biela's comet becomes explicable when we perceive of what slight materials the comet's train is formed. During the five hours of the display the earth tunnelled a path (as it were) through this train—a path a quarter of a million miles long, and having a circular section nearly 8,000 miles in diameter; and yet, in this enormous tubular section of the train, having a volume of about twelve millions of millions of cubic miles, there were but some thousands of scattered shreds of matter, so minute as to be unable to penetrate our atmosphere. Melted and vaporised high in the more tenuous regions of the atmosphere, these small bodies doubtless sank in the form of an impalpable powder to the surface of the earth, occupying many days, perhaps, in their descent.*

We are thus led to the consideration of certain circumstances in shooting-star displays which are essentially different from those which we took into account in dealing with meteorites and aërolites.

* A comet was detected by Mr. Pogson, Government Astronomer at Madras, near the part of the southern heavens towards which the meteors seen on November 27 were travelling. It was at first thought to be Biela's comet, but great doubt exists whether it was connected with that comet in any way.

It will be remembered that a part of the evidence respecting meteorites was based on the actual analysis, chemical and microscopical, of masses which have fallen upon the earth from interplanetary spaces. Shooting-stars do not fall as masses; and though their nature may be to some extent inferred from the spectroscopic analysis of their light, as well as from the consideration of the quantity of light which they emit, we are unable to apply to them the reasoning which had chief weight in the case of meteorites. We cannot assert that the substance of shooting-stars came originally from some star or sun in space, on the strength either of the molecular structure of those bodies or of their chemical condition, for we know absolutely nothing on either point. A shooting-star may, for aught that is known, contain, like meteoric iron, an excess of occluded hydrogen; or, when it first reaches our atmosphere, it may have that peculiar microscopical structure which has led Mr. Sorby to regard certain meteorites as expelled from glowing orbs like our sun and his fellow suns; but we have no means of ascertaining whether either condition prevails.

There are certain circumstances, however, about such meteor systems as the Leonides, Perseides, and these Bielan shooting-stars, which have a very decided bearing on the question of their origin.

If a meteoric mass were expelled from the sun it would return to the sun, unless either it encountered one of the planets, or else had been expelled with a velocity great enough to carry it for ever away from

the sun. For the latter event to happen, a velocity of about 380 miles per second must be imparted; and then the body would pass away not only from the sun, but from the solar system, and after a few millions of years had elapsed would pay a visit to some other star. After circling around this star, the body, if it escaped collision with one or other of the worlds circling around the star, and if its course carried it clear of the star itself, would pass away again, and enjoy another interstellar journey lasting for a few millions of years.

Of course, the same holds with meteors visiting our system from other stars. They reach the neighbourhood of the solar system after a journey lasting millions of years (even when they come from the nearest star): and if they escape entanglement in our system, they return to the interstellar depths, though not by the road which brought them here.

Now the shooting-star systems I have mentioned behave in quite another fashion. They travel round the sun in closed orbits and in periods of moderate length. Most of them do not approach the sun within fifty or sixty millions of miles, so as to exclude all possibility of their having once been expelled from the sun. For even if a body expelled from the sun, instead of returning directly to him, were enabled (through the effects of planetary perturbation) to steer clear of his mass, its course would nevertheless pass very near to him for ever after.

Whence, then, can these meteor streams come?

They cannot have come originally from the sun; and it is only by entanglement in our system that meteor systems coming from other suns can be compelled to circle around the sun. So that hitherto the theory has been accepted that these meteor systems are actually visitants from interstellar space, which have been entangled by the attraction of some one or other of the planets of the solar system, and so compelled to take up their present paths.

But it has recently been pointed out that enormous difficulties surround this theory. The giant planets, Neptune, Uranus, Saturn, and Jupiter, undoubtedly have power enough to compel a visitant from interplanetary space to circle in a closed orbit round the sun, if only the course of the said visitant carries him near enough to the particular giant planet which is to accomplish the work. But the approach must be very close indeed, or else the visitant will not be entreated to stay within our system. The matter is a very simple one. What induces the body to *visit* our solar system is the sun's attraction. This force acts on the body for millions of years before the body reaches even the outskirts of our system, and in the course of those years the body acquires an enormous velocity sunwards. This velocity would carry it, if undisturbed, close up to the sun, round whose orb the body would swiftly make a half-circular swoop, and would then be carried away by the action of the very same velocity which had brought it close to the sun. If this is to be prevented, it can only be by some planet depriving the body of a

certain portion of its velocity, and so taking away (in fact) its power of retreating.* To do this a planet must be peculiarly placed. It must be nearly on the course of the arriving body; but that is by no means sufficient: indeed the action of a planet so placed would simply be to accelerate the motion of the body as it approached, and it is precisely the contrary effect which is in the long run to be produced. A planet must be so placed that it draws nearer and nearer to the place where the body will cross, but not so near as to exert its most powerful action *until the body has passed that place.* *Then*, the body being *just inside* the planet's course, and travelling sunwards, the planet should be at its very nearest, and so exert its greatest pull in drawing the body *back.* The whole affair must be managed very neatly, so to speak; for the body will inevitably go past very quickly, and thus the planet has very little time during which to exert its power. Unless the planet gets a really effective pull, owing to the closeness of the body at the critical moment, the body will escape scot-free. And this applies quite as

* The case may be thus illustrated: When an elastic ball falls from a great height on a hard surface, it rebounds nearly to the height from which it had fallen. But suppose a netting to be interposed (not far from the hard surface), and that though the body bursts its way through the netting, it only does so at the expense of the best part of its velocity; then the body will not rebound to a great height, even though the netting be removed. A planet may, in like manner, interfere with a body approaching the sun from outer space; and though the body may get past the planet, it may only do so at the expense of a large portion of its velocity; and then instead of returning to the interstellar depths it will pass away only to a moderate distance from the sun (after its perihelion swoop).

forcibly to the greater giants, Saturn and Jupiter, as to the lesser giants, Neptune and Uranus; for though the two former have greatly the advantage in respect of attractive power, they have to deal with much more swiftly moving bodies. A visitant from the celestial depths would pass the orbit of Neptune with a velocity of only about five miles per second, while it would cross the orbit of Jupiter at the rate of about twelve miles per second. Now, if this were all, there would still be strong reason for doubting whether it really has been by the entanglement of visitors from interstellar space that meteor streams have been introduced into the solar system. Remembering that the orbits of the giant planets are from a thousand millions to 5,500 millions of miles in span, we see how enormous are the chances against one of these planets occupying just that short space (a few thousands of miles in length) of its orbit where it could act efficiently on a body crossing that orbit in its journey from outer space. But there is no reason whatever why a body so arriving should cross the orbit of any one of the planets. Indeed, the chances are enormously in favour of its path crossing the great plane of the planetary movements far away from any planetary orbit. Thus, even from the considerations already taken into account, we see that either there must be an inconceivably enormous number of visitants from outer space, or the giant planets would have but a poor chance of entrapping even a few meteors; whereas we know that the number of meteor streams existing within the solar system must be counted

by millions. But this is far from being all. To catch a single visitant from outer space is a task altogether easy by comparison with the task of catching a group of such visitants. Now, to take a single instance, the November meteor stream occupies at present a region many millions of times larger than the sun: it has supplied already many myriads of falling-stars, and at a very moderate computation it contains many billions of the bodies whose fall into our atmosphere produces meteoric displays. In its present condition, not the united power of all the giant planets could reverse the work originally (according to theory) executed by Uranus (one of the least of them), and send the November meteors away into outer space. But with every allowance for the process of dispersion which is assumed to have lengthened and widened and deepened this system, is it conceivable that it was ever so compact as to enable Uranus to execute the neat strategic movement by which he is supposed to have captured the whole flight? If Schiaparelli himself, who has taken a chief part in advocating the theory, is to be believed, a flight of meteors arriving from outer space would necessarily assume a cylindrical or lengthened shape long before it reached the neighbourhood of our system. In this case Uranus certainly could *not* have captured more than a small portion of the original flight of meteors, and we are left to form the most stupendous conceptions of that cloud of celestial visitants, if a mere fragment formed the November meteor stream. But taking only what we know, we see that the November meteor

stream is now a part of the solar system, and therefore, if Uranus ever acted in the manner supposed, he must have captured at least all the November stream at present recognised. That stream must once have been many million times more compact than it is at present; its associated comet must once have been so condensed (and in close company with the meteors) as to be utterly unlike the comet which Dr. Huggins examined with his spectroscope in 1866 ; and, in fine, added to all the marvels of the capture itself, we have to account for the most astounding changes of condition alike in comet and in meteor train.

To this it must be added that the same difficulties present themselves in accounting not only for all the known meteor systems, but for the myriads of meteor systems which must exist in order to explain (with any degree of probability) the fact that the small earth encounters so many. Thus it appears that Schiaparelli's ideas as to the *origin* of the meteor streams now forming part of our solar system are open to grave exception. It appears to me, indeed, that they are demonstrably untenable in at least the great majority of known instances.

We have thus been careful to exhibit the great difficulties which surround Schiaparelli's views,*

* It must be understood that our objections by no means extend to the important theory established by Schiaparelli, that meteors and comets are associated. It was in recognition of this theory, and certainly not in deference to his ideas as to the origin of meteors, that the Astronomical Society recently awarded its gold medal to Schiaparelli.

because the theory which we are about to describe seems at first sight too wild and fanciful for acceptance. It is only by showing the weakness of the only other theory thus far advanced that we can expect to claim attention for that which is now to be brought forward; though we hope to show that the new theory has strong independent evidence in its favour.

Since the November meteors (still to follow their history) were once very near to Uranus, for so much is certainly known, while nevertheless it is very difficult to believe that he captured them (acting as officer for the sun) in the condition of a very compact cloud or flight, no resource seems open to us but to believe that he expelled them from his own orb by some mighty eruptive action. And if we accept this view in the case of the November meteors, we must adopt a similar interpretation of the origin of meteor systems generally.

The new theory is then simply this, that, *the streams or systems which produce displays of shooting-stars, as well as the comets with which such streams are associated, were at some distant epoch expelled from the interior of one or other of the major planets which revolve outside the zone of asteroids.*

This theory sounds very startling at a first hearing. Yet let it be remembered that, (1) we have been almost forced to believe that meteoric masses have been expelled from our sun or his fellow suns; (2) it is almost certain

that the major planets were once in the condition of suns (even if they are not at the present time the scene of processes resembling solar action); and (3) if a large sun can expel matter from his globe with the enormous velocity necessary to carry such matter for ever away from him, it is more than conceivable that smaller suns should be competent to expel matter from their substance with the much smaller velocity necessary to free such matter from the attraction of the parent planet. It might also be added, that since we explain the downfall of heavy masses as due to solar eruptive energies, it appears reasonable to infer that the 'lighter metal' of the shooting-stars is due to the eruptive energies of minor suns. And it would appear to confirm this opinion that so far as observations have hitherto extended, all the meteoric streams which really belong to the solar system (and thus come under the above reasoning) produce displays of shooting-stars only, not casting down upon the earth any aërolitic masses, while all the comets which are of comparatively short period are in like manner of secondary importance as respects their dimensions.

Nevertheless, it is obviously desirable that a theory so surprising should be supported by independent evidence. It is, indeed, always a strong point in favour of a theory that it is one to which we appear to be driven by the failure of all others which had suggested themselves. But it is characteristic of a true theory that it not only meets the particular difficulties which have driven the reasoner towards it, but supplies an

explanation of other circumstances which had not hitherto been interpreted. All that is necessary to secure such a result is (ordinarily) to examine the new theory with care and attention, noting in particular the consequences which would follow if the theory were true. Let us see what follows from the adoption of such a course in the present instance.

Let us begin by supposing that at some far distant epoch, the four giant planets were all in the position of minor suns, erupting from time to time, and with great energy, masses of vaporous and molten matter, much (in their degree) as the sun is now understood to do. Then, whenever such masses were flung forth with sufficient force to overcome the attraction of the parent planet, they would forthwith revolve on an orbit round the sun. Now, those that the planet shot out in the same direction that itself was travelling in, would travel faster than the planet and follow a wider orbit. We should never see any traces of such masses, for they would always lie far beyond our range of view. It is reasonable to conclude that we should have the *best* chance of detecting those which were shot backwards so as to have their velocity as greatly reduced as possible. But without for the moment insisting on this, it is evident that for the ejected matter ever to come into *our* neighbourhood, it must have its perihelion (or place of nearest approach to the sun) somewhere near the earth's path, and its aphelion somewhere near the path of its parent planet. Only a small proportion of the ejections would be such as to

produce this particular result; but every one of the comets or meteor streams *known to us* should exhibit the peculiarity in question. Now, it is the fact that all the comets which, like Biela's and the comet belonging to the Leonides, have short periods, have their aphelia lying close to the orbits of one or other of the four giant planets. The peculiarity is not now noticed for the first time. It is so marked in the case of the comets dependent on the orbit of Jupiter, that they have been called Jupiter's comet-family. In the case of Neptune, again, there is a less numerous group of the kind, so well marked that, in the plan of their orbits given in Mr. Dunkin's supplement to Lardner's *Handbook of Astronomy*, the paths appear as though they had been purposely set in symmetrical adjustment with respect to Neptune's orbit.

Again, we may assume that Jupiter, who exceeds in mass the united mass of Neptune, Uranus, and Saturn, would vomit forth by far the greater number of these mixed masses of vaporized and molten matter. It is found, accordingly, that more than two-thirds of the comets which circle in closed orbits around the sun belong to the Jovian family of comets, the remaining third being distributed among the other three giant planets.

Yet although these circumstances agree satisfactorily with the new theory, they are not altogether convincing; simply because they might be expected to follow if the theory were sound which regards the giant planets as the disturbers of cometic masses arriving

from interstellar space. We come next, however, to a really crucial test (suggested to me by Professor Herschel, to whose consideration I had submitted the new theory). If Jupiter were to introduce a number of comets into our system by his action on matter arriving from without, we should recognise in these comets the signs of their extra-planetary origin, in a diversity of motion and direction corresponding to that recognised in the great comets which from time to time visit our system from without. Some of the Jovian family would travel forwards (that is, in the same direction as the planets), others would travel backwards, some would travel nearly in the level of the planetary motions, others on paths more or less considerably inclined, and some few on paths nearly square to that level. *But*, if the Jovian comet-family were originally expelled from Jupiter, inasmuch as they would all (besides the motion he gave on ejecting them) partake in his rapid forward motion, we should find them all travelling forwards and all on paths not greatly inclined to the level on which Jupiter himself travels. Precisely as when a bomb explodes in mid-air, the fragments all travel in the general direction which the bomb had before pursued, even though the bursting force acts backwards on some and sideways on others, so it would be with the matter expelled from Jupiter. Those cometic masses which we recognise as Jovian, may indeed (as suggested above) be masses hurled by Jupiter almost directly rearwards, yet that would still leave a balance of the forward motion they had shared

with him, and they would appear as comets having a direct, not a retrograde, motion.* And in like manner matter ejected even at a considerable inclination to the level in which Jupiter travels would yet travel nearly in that level, just as a ball which a passenger by an express train should attempt to fling straight upwards would in reality not travel vertically upwards but slantwise with respect to the ground.

It certainly seems to be a circumstance strongly favouring the new theory that this relation precisely accords with the observed peculiarities of the Jovian family of comets. It had been noticed respecting them (long before this theory had been thought of) that they *all* advance, and that they all travel on paths moderately inclined to the general level of the planetary motions. Sir John Herschel spoke thus in a lecture delivered in 1859, and published among his *Familiar Essays*:—'It is a very remarkable feature that *all* the comets of short period revolve in the *same* direction round the sun as the planets, and have their orbits inclined at no very large angles to the ecliptic.'

Now if we turn to Neptune, which travels at the rate of only three miles per second (while Jupiter travels eight miles per second), we should expect to find a different state of things. We might still expect,

* The case may be compared to that of water flung (not too sharply) over the stern of a swiftly advancing ship; such water would move backwards with respect to the ship, but seen from some station at rest would seem to move forwards. As Jupiter travels at the rate of eight miles per second, it is readily seen that matter retiring very swiftly *from him* rearwards would yet be advancing in reality.

indeed, to find the greater number of the Neptunian comets travelling forwards, but we could scarcely expect to find their paths lying nearly in the level of the ecliptic. Accordingly, out of six known Neptunian comets, we find that five travel forwards and only one (Halley's comet) travels backwards, while their paths are found to show every variety of inclination, one (De Vico's) having a path very nearly square to the level in which Neptune himself travels.

Adopting this theory of comets and their associated meteor trains, meteoric astronomy resumes something of the uniformity it possessed before the remarkable evidence was obtained which has recently compelled astronomers and physicists to regard some meteors at any rate as the results of solar explosions. All comets and all meteor systems, no matter what their grade, must be regarded as consisting of matter expelled from the various orbs which people space. *Only*, whereas the great meteoric masses owe their origin to suns, the streams of small meteors which produce the ordinary falling-stars owe their birth, if this theory be true, to the explosive energies of orbs which, like the giant planets of the solar system, lie midway in bulk and might between inhabited worlds such as the Earth, Venus, Mercury, and Mars, and great central suns like the orb which rules and nourishes our solar system.

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NEWS FROM THE MOON.

The Earl of Rosse, to whose father the world owes the telescope which turns its giant eye skywards from its underground home at Parsonstown, has recently published, in the Bakerian lecture of the Royal Society, the results of his successful efforts to measure the moon's heat. It is not my purpose to consider specially Lord Rosse's researches, which are indeed of such a nature as to be little suited for these pages. I propose rather, to avail myself of the attention just now directed to our satellite, in order to discuss some of the most remarkable and interesting facts which have been learned respecting the moon, and especially those which are least likely to be familiar to the general reader. But I cannot refrain from touching on a strange though not unexpected result which follows from Lord Rosse's researches. The cold, pale moon, that

> Climbs the sky
> So silently and with so wan a face,

has been shown to be in reality so warm, that no creature living on our earth could endure contact with that heated surface. The middle of the disc of the 'white full moon' is hotter than boiling water. It has thus been the fate of science yet once again to destroy an illusion which had for ages suggested a favourite poetical image. Poets will continue, indeed, to sing of the cold moon,

> Chaste as the icicle
> That's curded by the frost from purest snow,
> And hangs on Dian's temple;

but to the student of astronomy the contrast between the poet's fancy and the reality will mar the imagery.

The moon in her scientific aspect has been sufficiently coy, however. Notwithstanding her nearness and the seemingly favourable conditions under which we study her, very much less has been discovered respecting her than was anticipated when Galileo first observed

Imagined lands and regions in her orb.

She remains in many respects a mystery to us. We see little in her structure or aspect that is intelligible. Nevertheless, what has been learned is full of interest, even in its very strangeness, and in the perplexing problems which it suggests for our consideration.

Every one probably knows that the moon is nearly 240,000 miles from the earth; that she is about 2,100 miles in diameter (which is less than the earth's diameter about as 100 is less than 367); that the earth's surface exceeds hers about 13½ times, while the earth's volume exceeds the moon's about 49½ times. If to this I add that the moon is made of somewhat lighter material, or, to speak more exactly, that her mean density is somewhat less than the earth's, so that the earth exceeds her 81 times in mass or quantity of matter, I have indicated the principal circumstances which characterise the moon's globe as compared with the earth's. I shall have a word or two to add presently, however, about her probable shape.

We commonly regard the moon as a satellite of the earth, and we are taught at school and in our text-books, that while the earth travels round the sun, the

moon travels round the earth. But in reality this is erroneous, or is at least suggestive of error. The moon ought to be regarded as a companion planet, travelling with the earth around the sun. The distinction is not at all a fanciful one. The earth is not the body whose force the moon chiefly obeys. On the contrary, she is attracted more than twice as strongly by the sun. If the motions of the earth and moon could be watched from some far-distant standpoint, the observed movements would by no means suggest the idea that the moon was circling round the earth; and in fact, if the earth were concealed from view while her satellite was thus watched, the moon would appear to circuit round the sun in an orbit which could not be distinguished from that which the earth herself pursues. It is only from our earth as a standpoint that the moon seems to have the earth as the centre round which she travels; and to show how readily we may be deceived when so viewing any celestial body, we need only remember that, as seen from the earth, even the sun seems to have her as the centre of his motion. It is well to know the true nature of the moon in this respect; because when, instead of regarding her as merely a satellite or attendant upon the earth, we regard her as a companion planet — the least of the sun's inner family of planets—we perceive that in studying her we are making a first step towards the knowledge of other worlds than ours.

The most striking feature in the moon's telescopic aspect is the wonderfully disturbed condition of her

surface. Her face is scarred and pitted all over: nay, this but faintly expresses her condition, since no one can examine the moon carefully with suitable telescopic power without being impressed by the conviction that she has, so to speak, passed many times through the fire. There are great seams, as if at some early stage of her existence her whole globe had been rent apart by internal forces; and the duration of this early stage would appear to have been considerable, since there are several systems of these seams crossing and intercrossing. Then would seem to have come an age during which large regions sank as the moon cooled and contracted, leaving other regions elevated, as in the case of the great ocean valleys and continent elevations of our own earth. With further contraction came the formation of great corrugations, the lunar Alps and Apennines and other mountain ranges. But last of all, it may be presumed (if the recent results of Mallet's researches into vulcanology are to be accepted), came the most wonderful of all the stages of disturbances,—the great era of crater formation. One would say that the surface of enormous lunar tracts had bubbled over like some seething terrestrial substance, were it not that no materials known to us could form coherent bubbles spanning circular spaces many miles in diameter. Yet no other description gives so just an idea of the actual appearance of extensive tracts of the moon's surface, except *one*, equally or even perhaps more fanciful:—If the whole of one of these regions, while still plastic from intensity of heat, had been

rained upon by liquid meteoric masses many tons or even many hundreds of tons in weight, then something like the observed appearance would probably have resulted. Indeed, it is rather a strange circumstance that a fragment of a slab of green shale, pictured in Lyell's Geology, with casts of rain-prints left by a shower which fell ages on ages since, presents as true a picture of certain lunar tracts as a model cast expressly to illustrate what is seen in an actual photograph (moon-painted) of one of those regions. Whatever opinion may be formed as to the significance of this fact, it is certain that the present aspect of the crater-covered regions is quite inconsistent with the idea that there was a single continuous era of crâter formation. It is manifest that the contour of the whole surface has been changed over and over again by the forces which produced these craters.

Although we find little in the moon's aspect which reminds us of features at present presented by the surface of the earth, we must not too confidently assume that the two globes have been exposed to quite dissimilar processes of change. It is very difficult, indeed, to form clear ideas as to the real conformation of the earth's crust underneath those layers which have been formed, directly or indirectly, by the action of air and water. It requires but a slight study of geology to recognise how importantly such action has affected our earth. Indeed, there is not a square foot of the earth's surface which does not owe its present configuration either directly to weather changes and the action of water in

form of rain or snow or stream or flood, or else to processes such as vegetation or the succession of various forms of animal life. In the moon, so far as can be judged, we see the natural skeleton, as it were, of a planet, the rock surface precisely as it was left when the internal forces ceased to act with energy. There has been no 'weathering;' no wearing down of the surface by the action of water; no forests have formed carboniferous layers; no strata like our chalk formations have been deposited; vegetation does not hide any part of the surface; no snows have fallen, and therefore no glaciers grind down the rugged surface of the lunar valleys. With one exception, there is not, so far as can be judged, any process which is at work to disintegrate or modify the sterile face of the moon. The exception is the process of alternate expansion and contraction of the moon's crust, as the lunar day and night pass on in slow succession. Unquestionably, the change from a heat of some five hundred degrees at midday, to a cold far more intense than any with which we are acquainted on earth, must cause a gradual change in portions of the moon's surface.

But we are thus led to a most interesting question respecting the moon. It is manifest that now, at any rate, there is no water and very little air (if any) on the half of the moon turned towards us. Yet it is argued that those volcanic disturbances which are indicated so strikingly by the moon's aspect, imply the former existence both of water and of air. On our earth water appears absolutely necessary for the occurrence of

volcanic eruptions. Our leading seismologist, Mallet, lays down the rule, 'without water there can be no eruption,' and it was long since pointed out by Humboldt that all the active volcanoes of the earth are close to the sea. Of course the chief evidence in favour of this view consists in the nature of the substances emitted during eruptions; and, in point of fact, the view may be regarded as a demonstrated *terrestrial* relation. Then it is quite impossible to conceive that so many and such violent eruptions as the lunar volcanoes indicate can have taken place without the emission of quantities of vapour so enormous that a discernible atmosphere would, from that cause alone, have been formed around our moon. The carbonic acid gas, for example, which would be poured out if the lunar volcanoes in any degree resembled ours, would form a gaseous envelope of no inconsiderable depth. This will be manifest when we recall Galileo's description of the lunar craters as resembling the eyes in a peacock's tail for number. Besides, it is difficult to imagine how any planet could be formed without an atmosphere; and although, no doubt, the moon's small mass would indicate a very inconsiderable aërial envelope, yet it would not explain the complete absence of all traces of air.

The considerations here mentioned have long formed one of the standing mysteries of astronomy. We see in our moon a planet which ought to have oceans and an atmosphere, which even would seem once to have had them; and yet she now shows no trace of either.

The efforts made to explain the matter have been sufficiently strenuous.

Whiston suggested that a comet had swept away the lunar air and oceans, a view the more remarkable because he held the theory that our own oceans had been formerly recruited by a comet which produced a universal deluge. Of course, what is now known about comets will not permit us for a moment to entertain the supposition that one of these bodies could carry off any portion of the moon's belongings. A comet might rain a shower of meteoric stones upon the moon, and so recruit her mass: indeed, the idea has been suggested of late that this happened repeatedly in those far-off ages when all the planets were exposed to such influences, their 'growing mass,' as Wendell Holmes says,

Pelted with star-dust, stoned with meteor-balls.

That the moon should borrow from comets is not unlikely therefore, but that comets should rob the moon is altogether improbable.

There is another theory scarcely less fanciful. It has been suggested that the moon has grown intensely cold. Her small orb, though once instinct with fire, has long since parted, according to this theory, with all its inherent heat. All the forms of life that once existed on the moon, animal life, vegetable life, and the life which our imagination pictures where great natural changes are in progress, have been, so to speak, frozen out. The moon's oceans have congealed to their utmost depths. The very gases which once formed her atmosphere have frozen, until at last she has become

the dead globe we see, never to be warmed again into life, and having no other use in the economy of the universe but to illuminate our earth and regulate her tides.

But while it is quite conceivable that the intensity of cold during the long lunar nights may be amply sufficient to turn every gas we know of into the solid form, it is manifest that the intense heat to which the moon is exposed during her equally long day would produce even more remarkable changes when poured upon such a frozen surface than it would effect on such a globe as our earth in its present condition. Imagine our oceans frozen, and the air also frozen, so as to lie in great drifts many feet deep* over the whole surface of the globe. Then conceive the sun to pour his rays down upon that frozen surface for a day lasting two of our weeks, his midday place being nearly overhead. Is it not manifest that the frozen air would be melted and vaporised (turned, that is, into our familiar air) and then the ocean melted, and enormous quantities turned into vapour. Such are the actual conditions in those lunar regions which form the middle of the moon's face. Yet at the time of full moon no signs of change can be recognised, at least none which correspond to

* We do not know the actual depth, because we do not know what is the density of solid oxygen or solid nitrogen. But we know that if the density of these elements, when reduced to the solid state, were equal to that of ice, the atmosphere would be converted into a solid layer, more than thirty feet deep, for the water-barometer stands at more than thirty feet. If frozen oxygen and nitrogen are as dense as mercury, then the layer would be only two and a half feet in depth.

the vaporisation of a frozen atmosphere, and of frozen oceans. The simple fact, however, that Lord Rosse's experiments prove that the full moon is greatly heated, disposes at once of the fanciful theory we have been considering. For a frozen lunar atmosphere could not be heated beyond the point (corresponding to an exceeding cold) where it becomes gaseous, until the whole of it had assumed this form; and after that, the water under the atmosphere could not be heated above boiling heat without turning altogether into steam. Now of two things one. The boiling heat would be either high or low. If high that would imply considerable atmospheric pressure, and we could not but recognise an atmosphere producing such pressure; if low, then the degree of heat to which the moon is raised, as Lord Rosse's experiments show,* remains altogether inexplicable.

There is another strange theory in explanation of the absence of water and air in the moon, due to Dr. Frankland. According to this theory, the oceans and atmosphere which once existed on the moon have now withdrawn into the moon's interior. 'If water at one time existed on the surface of the moon,' says Frankland, 'whither has it disappeared? If we assume, in accordance with the nebular hypothesis, that the portions of matter composing respectively the earth

* Lord Rosse separates the effect of reflected sun heat from that heat which the moon emits as a warmed body. We do not explain here the principles which render it possible to distinguish between these two forms of heat; but their sufficiency is altogether beyond question.

and the moon once possessed an equally elevated temperature, it almost necessarily follows that the moon, owing to the comparative smallness of her mass, would cool more rapidly than the earth. This cooling of the moon's mass must, in accordance with all analogy, have been attended with contraction, which can scarcely be conceived as occurring without the development of a cavernous structure in the interior. Much of the cavernous structure would doubtless communicate, by means of fissures, with the surface, and thus there would be provided an internal receptacle for the ocean, from the depths of which even the burning sun of the long lunar day would be totally unable to dislodge more than traces of its vapour. Assuming the solid mass of the moon to contract on cooling at the same rate as granite, its refrigeration through only 180 degrees of the Fahrenheit thermometer (the difference between the boiling heat and the freezing point) would create cellular space equal to nearly 14½ millions of cubic miles, which would be more than sufficient to engulf the whole of the lunar oceans, supposing them to bear the same proportion to the mass of the moon as our own oceans bear to that of the earth.'

Room might certainly be found in this way for all the lunar oceans, because the moon's surface amounts only to 14,600,000 square miles, and therefore the cellular space deduced above amounts to the volume of an ocean competent to cover the whole surface of the moon to the depth of a mile. But then, where has the lunar atmosphere gone to? It would require

much more room than the oceans, if originally comparable to our own atmosphere in density. For even at a height of 22 miles from the moon's surface the density of the air would only be reduced one-half, so that half the lunar air would occupy a shell of space covering the whole moon to a depth of 22 miles. It would thus require 22 times as much space as Frankland's theory gives, and still the other half would be left outside the moon. But even the oceans are not very easily accounted for on this theory. We must assume that when they existed *on* the moon's surface they were not quite so hot as boiling water on the earth. In fact Frankland's theory depends in great part on the probable existence of glaciers on the moon, and it need hardly be said that there would be no glaciers while the oceans, and therefore the solid moon, were at the temperature of boiling water. How then is the refrigeration through 180 degrees to take place without passing far below the freezing point? But frozen oceans would assuredly not find their way into the moon's interior through the fissures of Frankland's theory. Apart from this it must be remembered that if the moon had a very rare atmosphere, the boiling point would be very much lower than on the earth; while if she had an atmosphere as dense as ours, it remains impossible to understand where that atmosphere can have gone to.

I have said that the theory requires that formerly glaciers should have existed on the moon. It is manifest that, apart from the theory, the question whether

there were ever any glaciers on the moon is full of interest. For if there were glaciers there must have been snow and rain, as well as wind currents to bear the moisture-laden air against the slopes of the lunar mountain ranges. It will be well, therefore, to indicate the evidence which Frankland finds for the lunar glaciers of his theory. "What may we expect to see?' he says. 'Under favourable circumstances the terminal moraine of a glacier attains enormous dimensions; and consequently, of all the marks of a glacier valley, this would be the one most likely to be first perceived. Two such terminal moraines, one of them a double one, have appeared to observers to be traceable upon the moon's surface.' His description of the position of these would not be intelligible without a lunar chart; but students of the moon will understand where to look for them when we mention simply that one lies near the end of the remarkable streak from Tycho* to Bullialdus, crossing this streak exactly opposite Lubiniezky, while the other lies at the

* Tycho is that spot where the full moon shows a gathering together of streaks, somewhat as at either core-end of a peeled orange. Indeed, small photographs of the full moon look so much like photographs of a peeled orange that, as Wendell Holmes notes, many persons suppose astronomers have substituted the orange for the moon, so as to save themselves trouble. Imagine how pleasing such an idea must be to our De la Rues, Rutherfurds, and others, who have exhausted the contrivances of mechanical ingenuity to make their great telescopes truly follow the moon, and have devised at infinite labour the best photographic appliances to secure good results. It is only right to say, however, that no one would for a moment mistake the masterpieces of these astronomers for photographs of a peeled orange, since they are equal in distinctness to views of the moon with excellent telescopes.

northern extremity of the lunar valley which runs past the eastern edge of Rheita. Describing the first, Frankland says, there are 'two ridges forming the arcs of eccentric circles. Beyond the second ridge a talus slopes down gradually northward to the general level of the lunar surface, the whole presenting an appearance reminding the observer of the concentric moraines of the Rhone glacier. These ridges are visible for the whole period during which that portion of the moon's surface is illuminated; but it is only about the third day after the first quarter, and at the corresponding phase of the waning moon, when the sun's rays, falling nearly horizontally, throw the details of this part of the moon's surface into strong relief, and the appearances suggest this explanation of them.' It will be manifest that the evidence for glaciers on the moon is not altogether irresistible. On the whole face of that hemisphere, seven millions of square miles in extent, which the moon turns earthwards, there are but two spots where appearances are recognised which suggest the idea of glacial moraines. This is not convincing, especially when we remember that under the best telescopic scrutiny yet applied to the moon we see her surface only as we should see a mountain region on the earth from a distance of more than one hundred miles, and through a dense and perturbed atmosphere. For all the atmospheric effects are multiplied precisely in proportion to the power of the telescope employed, so that even when we use so high a power as 2,400, which

would theoretically reduce the moon's distance to one hundred miles, the atmosphere between us and the moon is, as it were, multiplied 2,400 times.

But we have not even yet exhausted all the ingenious theories which have been devised by those who insist on endowing the moon of former ages with oceans and an atmosphere. We have seen a comet called in to carry away the lunar air and water; next we have had them frozen up; and thirdly, the moon's interior has opened to remove them from our sight. But a fourth theory remains, which, though not less startling than the others, has found singular favour even among astronomers of repute. According to this fourth theory, the lunar oceans and atmosphere have withdrawn, not into the inside of the moon, but to her other or unseen side. The farther half of the moon is never seen by us, and being unknown has appeared to afford a favourable opportunity of applying the principle 'omne ignotum pro mirifico.' Accordingly, it has been supplied with oceans and an atmosphere, in fact with a double quantity of air and water; inhabitants are, of course, not wanting where circumstances are so suitable for their subsistence; and in fine, another world* exists on the unseen half of the moon.

It would be unfair, however, to describe this theory as though it were merely based on our ignorance of the state of things on the farther side of the moon,—as though, in fact, it resembled one of the *peut-êtres* of Fontenelle (who was an ardent believer, by the way,

in the habitability of our satellite). The theory was originally suggested by a mathematical inquiry of singular profundity. The skilful German mathematician, Hansen, found reason to believe that if the moon's centre of gravity is not exactly at the middle point of that diameter of hers which is directed earthwards, her movements must give evidence of the fact. If the centre of gravity were farther away than the middle point she would show a slight peculiarity of motion in one direction, while if the centre of gravity were nearer than the middle point she would show a peculiarity of the opposite kind. On examining the moon's actually recorded motions, Hansen considered that he had evidence sufficing to prove that the centre of gravity is more than thirty miles farther away than the middle point just mentioned. Now clearly, if the moon's *shape* is very nearly globular, but she is like a loaded die, heavier on one side than the other, her oceans and atmosphere must pass over to the loaded side. To use the emphatic mode of describing matters employed by Sir John Herschel in a letter to the present writer, the farther side of the moon, according to Hansen's view, is 'like a great lake basin, nearly forty miles deep.' Of course, Herschel did not mean that there is a great concavity on that side, any more than a geographer would mean that the ocean bottom is concave, if he spoke of the ocean *basin*. But the state of the farther side of the moon, according to the theory we are considering, is precisely as though matter were excavated away to a depth of nearly forty miles,

leaving, of course, ample room for every drop of water to flow to that unseen half. The air would also flow to that side. It is not, however, altogether so clear that the air would be concealed in the same way that the water would be. The fact is, one half of the moon is *not wholly* hidden from our view. There is a 'balancing motion' (technically called the 'libration') of the moon, by which she now tilts one part of the farther hemisphere towards the earth, and then another part, with a singular alternation which brings the balancing round so as to affect in turn every part of the moon's edge. And owing to this peculiarity, instead of one half of the moon remaining concealed from us, about forty-two parts out of 100 only are altogether and at all times unseen. It is difficult to believe that an atmosphere coerced so much less than our own (since the moon's attractive power at her surface is but one-sixth of the earth's at her's) would confine itself strictly within limits so narrow.

But in reality, evidence has been obtained in favour of Hansen's fundamental theory, which, if admitted, disposes altogether of the conclusions based upon that theory. The continental astronomer, Gussew, of Wilna, has very carefully examined some of De la Rue's lunar photographs, taken when the moon was at opposite stages of her balancing motion, and by noting how much the several craters, &c., are displaced, he has found the means of determining the shape of the moon's surface. According to his measurements the greater part of the visible surface of the moon must be

regarded as an enormous elevation, rising in the middle fully seventy miles above the mean level. In fact, the moon, according to these measurements, would come to be regarded as egg-shaped, the smaller end of the egg being turned earthwards,—only it will of course be understood that, regarded as a whole, the moon's body would not differ very markedly from the globular form. It would be shaped, to speak plainly, like a nearly round egg.

Of course, this way of throwing the centre of gravity farther away than the middle of the lunar diameter directed towards the earth, leads to results quite different from those which would follow if the moon were a globe in shape but loaded like a die internally. That great hill of matter on the earthward side of the moon would draw the oceans and air *away* from the farther side—not, indeed, to its own summit, that is, not to the middle of the disc we see, but to its base. In fact, there would be a gathering of the waters in a zone all round the edge of the moon's visible disc, and over this zone the atmospheric pressure would also be greatest. Since, as a matter of fact, there is no sign either of water or air on this zone of the moon's surface, we must perforce abandon the theory that lunar oceans and air still lie anywhere on the surface of the moon.

The reader will probably conclude, as the evidence seems to require, that all ideas to the contrary notwithstanding, the moon has never had either a watery envelope or an aërial one in the slightest degree comparable in relative extent with those on our earth.

But before we pass to the curious questions suggested by the manifest signs of violent volcanic action on the moon in former ages, when neither water nor air existed in any considerable quantity, let us pause for a moment to discuss the remarkable result attained by Gussew.

If we suppose that there really is a bulging out on the earthward side of the moon, to the enormous extent indicated by Gussew's measurements, we have a singular problem to inquire into. For theoretically, as Newton showed long since, the moon ought to be in shape what geometricians call an ellipsoid. The earth's globe is slightly flattened one way, and we call such a figure a spheroid; but now suppose that besides being compressed at the poles, she were also (as some think she actually is) compressed (but to a much smaller degree) at two opposite parts of the equator, so that the equator itself was slightly oval. Then she would have her shortest diameter, as now, the polar one; her longest diameter would be the longest diameter of her oval equator; and she might be said to have an intermediate diameter, viz., the shortest diameter of her equator. So it should be, says Newton, in the case of the moon. She should be most compressed at the poles, or nearly at the north and south points of her disc; her longest diameter should be the one turned towards the earth; and a thwart diameter lying nearly east and west would be her third or intermediate diameter. Then he calculated the length of these several diameters, and found that the shortest

would not differ more than sixty-two yards from the longest. This is something very different from the seventy miles resulting from Gussew's measurements.

If then that monstrous hill exists, we must look for its origin in some extraneous cause, since we see that a globe assuming its natural figure under such conditions as prevailed in the moon's case would present no such excrescence. I believe I am justified in saying that the photographic evidence is accepted by Dr. De la Rue himself. In fact, when two pictures of the moon, in opposite stages of her balancing, are looked at, the stereoscopic view shows Gussew's great hill actually standing out, as it were, before the very eyes. I venture to quote Sir John Herschel's account of the principle of this method, because of the singularly effective way in which he presents the matter. He says: 'Owing to the libration of the moon, the same point of her surface is seen sometimes on one side of the centre of her disc, and sometimes on the other, the effect being the same as if, the moon remaining fixed, the eye were shifted from right to left through an angle equal to the total libration. Now this is the condition on which stereoscopic vision depends, so that by choosing two epochs when the moon is presented in the two aspects best adapted for the purpose, and taking separate and independent photographs of it in each aspect, the two, stereoscopically combined, so completely satisfy all the requisite conditions as to show the spherical form *just as a giant might see it, whose stature was such that the*

interval between his eyes should equal the distance between the place where the earth stood when one view was taken, and that to which it would have been removed (the moon being regarded as fixed) to get the other. Nothing can surpass the impression of *real corporeal form* thus conveyed by some of these pictures as taken by Dr. De la Rue with his powerful reflector, the production of which (as a step in some sort taken by man outside of the planet he inhabits) is one of the most remarkable and unexpected triumphs of scientific art.'

Both the measurement and the simple contemplation of the stereoscopic pairs of lunar pictures appearing to indicate the same result, we may proceed to inquire under what circumstances that result may have been brought about. The true explanation can scarcely fail to be a singular one, whatever it may be; so that if we are led to a view which appears sensational, this must not be regarded as a surprising circumstance.

Now let it be noted that, whatever ideas we may form as to the past condition of our earth and the other members of the solar system, we can scarcely refuse to admit the general theory that in long-past ages every one of these globes was in a condition of intense heat. That our earth was formerly liquefied by intensity of heat is the opinion of all who have carefully studied her surface; and there are few men of science who do not, after examining the evidence, conform to the theory of Meyer, that the earth was formerly in a

vaporous condition. Assuming that, as our poet laureate has expressed the theory—

> This world was once a fluid haze of light,
> Till toward the centre set the starry tides
> And eddied into suns, that wheeling cast
> The planets—

we can form no other conception of our earth's primal condition than as a vapour globe. Our moon likewise affords abundant evidence of having once been in an intensely heated state. And doubtless there was once a time when the earth and moon were both (at the same time) vaporous through intensity of heat.

Now, I have not gone back to that far distant epoch for the purpose of seeking there for the secret of the moon's present figure. It appears to me reasonable to trace back to such an epoch the singular law of the moon's rotation, whereby she always keeps the same face turned towards the earth; for, far off though that epoch may be, it is not separated from our time by so enormous a lapse of ages as could be required to 'brake' a rapidly rotating moon to the moon's present strangely slow rotation-rate. In the distant era, then, when the moon was a vapour-nucleus within the great vapour-globe which was at some future period to form the earth we live upon, the moon thus involved learned to rotate synchronously with her revolution. But gradually the earth's vapour-globe shrunk in its dimensions until the moon was left outside—or we may say that the vaporous envelopes around the two chief nuclei so far shrank as no longer to be anywhere intermixed. From this time forth the moon

must have cooled more rapidly than the earth; and the time must at length have arrived when the moon had become an opaque orb, while the earth on which we live was still a sun. Even at this early stage of our existence the moon must have so rotated as to turn the same face towards the earth's then glowing orb.

But now a circumstance has to be considered which, startling though it may seem at first, is yet consistent with what has been ascertained respecting the sun and other bodies. There is a great mass of evidence tending to show that our sun expels matter from his interior with a velocity sufficient to carry such matter entirely away from him. This has been shown by the microscopic and chemical structure of meteorites, by their paths and rates of motion, and by many circumstances which will be found detailed at length in the article called 'Meteors, seed-bearing and otherwise,' in the present volume. It is also very strikingly supported by the behaviour of the so-called eruption-prominences of the sun. Passing from the sun to the major planets—which even now seem to have some of the qualities of subordinate or secondary suns, and must certainly have been such long after the earth and her fellow minor planets had cooled down into the condition of habitable worlds—we find very striking evidence to show that these minor suns or major planets erupted from their interior the material of meteor systems and of those comets of small period which have been called the comet-families of the major

planets. The evidence on this point will be found fully detailed in the article called 'The Recent Meteor Shower and Meteor Showers generally,' which precedes the present. And the circumstance will there be found noted, that we need not inquire into the dimensions of a body in considering the possibility of its expelling matter from its interior with a velocity sufficient to carry such matter altogether away; since, in point of fact, the inferiority (for instance) of the major planets compared with the sun, is compensated by the inferior attractive power which their eruptional forces have to overcome. All that is required is a sunlike condition with respect to heat; granting this, a small globe like the earth, or even so small a globe as the moon, would be as competent to expel matter to great distances from its interior as the major planets, or as the sun himself, or even as an orb like Sirius, exceeding our sun at least a thousand times in volume.

So long then as our earth continued in a sunlike state, she would probably expel matter in all directions with a velocity small indeed compared with the velocity of matter erupted from the sun, but quite as large relatively to the attractive power of the earth. This process of continual eruption would not exhaust the earth, simply because it would be compensated by arrivals from without; and moreover, far the greater quantity of the erupted matter would doubtless fall back upon the glowing orb of the earth. But it is manifest, that whatever matter was erupted directly

towards the moon, so as to fall upon her, would recruit her mass. As we must assume from the known mass of the earth that she was for ages in a sunlike condition, we must believe that during those ages that face of the moon which was continually directed earthwards received no inconsiderable supply of erupted matter. For it must be remembered that when the process began the moon was much larger in volume, though considerably less in mass, than at the present time. She would, therefore, at that time intercept a much greater proportion of the erupted matter. Moreover, since, after she had shrunk into a semiplastic but still growing orb, the moon must have continued for a very long time subject to this rain of earth-born missiles, there is reason for regarding as very considerable the quantity of matter by which her bulk was thus increased. Moreover, if it be remembered that the meteoric missiles thus expelled from the earth would necessarily be exceedingly hot, probably liquid even before their fall, and certainly liquefied at the moment of collision with the moon's surface, we find *à priori* evidence for that very downfall of liquid drops, of which, as mentioned above, the present aspect of the moon seems to afford evidence. It is certainly a noteworthy circumstance that a theory devised to explain a most striking peculiarity of the moon's globe, should account also for a feature, not less striking, which had not been specially in view when the theory was invented.

We must pass, however, from these considerations, because the evidence on which they have been based is

too slight to warrant any prolonged or exact discussion respecting them. But a few words remain to be said on the question which originated the strange theories devised to explain why the moon at present shows no traces either of oceans or an atmosphere.

I have said that on our earth the law seems established that where there is no water there are no volcanoes. May it not be, however, that this law does not extend to the moon? Mr. Mathieu Williams, whose work, *The Fuel of the Sun*, has suggested many new and striking considerations respecting the celestial orbs, has brought to bear on this question an experience which very few students of astronomy have possessed—the knowledge, namely, of the behaviour of fused masses of matter cooling under a variety of circumstances. 'I have watched the cooling of such masses very frequently,' he says, 'and have seen abundant displays of miniature volcanic phenomena, especially marked where the cooling has occurred under conditions most nearly resembling those of a gradually cooling planet or satellite—that is, when the fused matter has been enclosed by a resisting and contracting crust. The most remarkable that I have seen are those presented by the cooling of the "tap cinder" from puddling furnaces. This, as it flows from the furnace, is received in stout iron boxes (called "cinder bogies"). The following phenomena are usually observable on the cooling of the fused cinder in a circular bogie. First a thin solid crust forms on the red-hot surface. This speedily cools sufficiently to blacken. If pierced by a slight thrust from an iron rod, the red-

hot matter within is seen to be in a state of seething activity, and a considerable quantity exudes from the opening. If a bogie filled with fused cinder is left undisturbed, a veritable spontaneous volcanic eruption takes place through some portion, generally near the centre, of the solid crust. In some cases, this eruption is sufficiently violent to eject small spurts of molten cinder to a height equal to four or five times the width of the bogie. The crust once broken, a regular crater is rapidly formed, and miniature streams of lava continue to pour from it; sometimes slowly and regularly, occasionally with jerks and spurts, due to the bursting of bubbles of gas. The accumulation of these lava-streams forms a regular cone, the height of which goes on increasing. I have seen a bogie about ten or twelve inches in diameter, and nine or ten inches deep, surmounted in this way by a cone about five inches high, with a base equal to the whole width of the bogie. *These cones and craters could be but little improved by a modeller desiring to represent a typical volcano in eruption.*'

The aspect of the moon's crater-covered surface certainly accords better with the supposition that active processes like those described by Mr. Williams were in operation when that surface was formed, than with the theory that slow and intermittent volcanic action like that with which we are *now* familiar on earth, modelled the moon's surface to its present configuration. In the former case water would not have been needed, and

vaporous matter would not have been expelled to an extent irreconcilable with observed phenomena.

It is manifest that we have in the moon a subject of research which has been by no means exhausted. Ascertained facts respecting her have not yet been explained; and doubtless many facts still remain to be ascertained. The moon will hereafter be examined with greater telescopic power than has yet been applied, and when this has been done appearances may be accounted for which are at present unintelligible. Again; new inquiries into the question of the evolution of our solar system can hardly fail to throw light on the peculiar relations presented by the moon with reference to the terrestrial globe. I believe that the problems suggested by lunar research, perplexing though they unquestionably are, will not be found insoluble; and it is most probable that their solution will in turn throw important light on the history of our earth and her fellow terrestrial planets, on the giant planets which travel outside the zone of asteroids, and lastly, on the past history, present condition, and future fate of the great central luminary bearing sway over the planetary system.

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EARTHQUAKES.

It is related in the *Timœus* of Plato that the ancient Egyptians held the world to be liable to occasional widely extended catastrophes, by which the gods checked the evil propensities of men, and cleansed the earth from guilt. Conflagrations, deluges, and earthquakes were the instruments of the wrath of the offended gods. After each catastrophe mankind were innocent and happy, but from this state of virtue they gradually fell away until their accumulated offences called for new judgments. Then the gods took counsel together, and unable to bear with the multiplied iniquities of the human race, swept them from the earth in some great cataclysm, or sent a devouring flame to consume them, or shook the solid earth until hills and mountains fell upon and crushed the inhabitants of the whole world.

One can understand how the confused records of great catastrophes, in which all, or nearly all, the inhabitants of wide districts were destroyed, led in the course of time to the formation of such views as Plato has described. And, indeed, it is not in one nation alone that we find theories of this sort prevalent. In the *Institutes of Menu* the Hindoos are taught that at the end of each of those cycles of ages which are termed the 'days of Brahma' all forms of life are destroyed from the earth by a great conflagration, followed by a deluge which inundates heaven itself.

The mythical legends of the Chinese refer to similar views, which appear also in the Babylonian and Persian cosmogonies. The Chaldeans taught that when the planets are all conjoined in Capricorn the earth will be overwhelmed by a flood, and that when a conjunction of this sort takes place in Cancer the earth will be destroyed by fire.

In the present age when the network of telegraphy brings all parts of the earth into close intercommunication, we are not likely to trace, even in the most wide-spread disasters, the approaching destruction of our globe. The same day which brings the intelligence of some desolating catastrophe brings evidence also that the devastation is but local. We are seldom informed of simultaneous, or nearly simultaneous, events happening in widely-separated regions of the earth's surface. Accordingly, we are seldom led to dread the occurrence of any widely-devastating series of catastrophes.

But certainly events have happened during the past few months which might lead nervously-disposed persons to imagine that the inhabitants of the earth are not perfectly safe from wide-spread destructive agencies. The same week that brought news of the great hurricane which ravaged the West Indian Isles brought also the account of destructive hurricanes in the Indian and Pacific Oceans. Then followed the series of earthquake shocks which have inflicted such injury on the already much-tried inhabitants of St. Thomas, and which still continue to be felt at intervals.

Next we hear of an earthquake in Somersetshire, then in Malta, then in Egypt, then at Formosa, then in St. Salvador; and now, almost as I write, the bed of the Pacific is violently shaken, and hundreds of the inhabitants of the Sandwich Islands are destroyed by a violent uprush of molten matter. During all this time Vesuvius has continued in violent eruption.

Thus it has happened that we have heard a great deal lately of certain speculations—recently ventilated by an American philosopher—which threaten the earth with complete annihilation. According to these views there is one great danger to which we are at all times liable—the risk, namely, that some large volcanic vent should be formed beneath the bosom of ocean. Through this vent the sea would rush into the interior of the earth, and being forthwith converted into steam by the intense subterranean heat, would rend the massive shell on which we live into a thousand fragments.

Whether it is possible or not that such an event as this should take place, I shall not here stay to inquire. Let it suffice that the risk—if there be any—is no greater now that it has been any time during thousands of past years.

But certainly, if there is any source from which the inhabitants of the earth may reasonably dread the occurrence of widely devastating catastrophes, it is from earthquakes. It is related that for full six months after the great earthquake of Lisbon, Dr. Johnson refused to believe in the occurrence of so terrible a catastrophe. 'He spoke half jestingly,' Macaulay

thought—it is not easy to see on what grounds. To us it seems far more probable that Johnson heard with natural wonder and awe of the destructive effects of this fearful convulsion; and that for awhile he could scarcely believe that the extent of the disaster had not been exaggerated. It would be well if, indeed, the powers of earthquakes were less tremendous than they have been repeatedly shown to be. 'There is,' says Humboldt, 'no other outward manifestation of force known to us—the murderous inventions of our own race included—through which in the brief period of a few seconds or minutes, a larger number of human beings have been destroyed than by earthquakes.' Lightning and storm, war and plague, are but weak and inefficient agents of destruction in comparison with the earth's internal forces.

And as earthquakes surpass all other phenomena as agents of sudden destruction, so the impression which they produce on those who for the first time experience their effects is peculiarly and indescribably awful. Men of reputed courage speak of a feeling of 'intolerable dread' produced by the shocks of an earthquake, 'even when unaccompanied by subterranean noises.' The impression is not that of simple fear but a feeling of absolute pain. The reason seems for awhile to have lost the power of separating real from imaginary causes of terror. The lower animals, also, are thrown into a state of terror and distress. 'Swine and dogs,' says Humboldt, 'are particularly affected by the phenomenon of earthquakes.' And he adds that 'the very

crocodiles of the Orinoco, otherwise as dumb as our little lizards, leave the shaken bed of the stream and run bellowing into the woods.'

Humboldt's explanation of the peculiar sensations of alarm and awe produced by an earthquake upon those who for the first time experience the effects of the phenomenon is in all probability the correct one. 'The impression here is not,' he says, 'the consequence of the recollection of destructive catastrophes presented to our imagination by narratives of historical events; what seizes us so wonderfully is the disabuse of that innate faith in the fixity of the solid and sure-set foundations of the earth. From early childhood we are habituated to the contrast between the mobile element water and the immobility of the soil on which we stand. All the evidences of our senses have confirmed this belief. But when suddenly the ground begins to rock beneath us, the feeling of an unknown mysterious power in nature coming into operation and shaking the solid globe arises in the mind. The illusion of the whole of our earlier life is annihilated in an instant.'

Use habituates the mind to the shocks of earthquake. Humboldt found himself able after awhile to give a close and philosophic scrutiny to the circumstances attending the phenomenon which had at first impressed him so startlingly. And he tells us that the inhabitants of Peru think scarcely more of a moderate shock of earthquake than is thought of a hail-storm in the temperate zone.

Yet the annals of earthquakes are sufficient to give rise to a feeling of dread, founded, not merely on the novelty of the event, but on a knowledge of the powers of the earth's internal heavings. The narratives of some of the great earthquakes afford fearful evidence on this point.

In the first shock of the great earthquake of Lisbon (November, 1755) the city was shaken to its foundations. The houses were swung to and fro so violently that the upper stories fell at once, causing a terrible loss of life. Thousands rushed to the great square in front of St. Paul's Church, to escape the reach of the tottering ruins. It was the festival of All Saints, and all the churches had been crowded with worshippers. But when the terrified inhabitants reached the square they found that the great church of St. Paul's was already in ruins, and the immense multitude which had thronged its sacred precincts were involved in its destruction. Such of the congregations of the different churches as had escaped rushed to the banks of the Tagus for safety. There were to be seen priests in their sacerdotal vestments, and an immense crowd of people of all ranks and ages, praying to Heaven for mercy. As they prayed there came the second shock, scarcely less terrible than the first. The church on the top of St. Catherine's Hill was rocked to and fro till it fell, crushing in its fall a great multitude which had sought that height for safety.

But a far more terrible catastrophe was at hand. As the banks of the river resounded with the *Miserere* of

the terrified supplicants who had crowded thither for safety, there was seen to pass over the wide expanse of the stream (here four miles broad) a strange heaving swell, though no wind stirred the air. The waters seemed to be drawn away to meet a vast wave which was now first observed to be bearing down upon the devoted crowd. They strove to fly but the wave swept too rapidly onwards. The whole multitude was overwhelmed in a moment. A magnificent quay, lately built at a great expense, was engulfed with all who had crowded on it for refuge. Numberless vessels, also, which were anchored on the river and were now full of terrified people—seeking on an unstable element the security which the solid earth denied them—were sucked down by the tremendous wave and not a trace of them was ever afterwards seen.

A third shock followed, and again the river was swept by a gigantic wave. So violently was the river moved that vessels which had been riding at anchor in deep water were flung upon the dry ground. Other shocks and other inroads of the river-water followed, each working fresh destruction, insomuch that many began to believe that 'the city of Lisbon was doomed to be entirely swept from the face of the earth.'

It would be out of place to describe here at length how fire and pestilence came successively to complete the desolation begun by the earthquake's ravages. The terrible story has been narrated elsewhere. But what remains to be mentioned gives us startling evidence

of the terrible energy of the earth's subterranean forces:—

The mountains Arrabida, Estrella, Julio, Marvan, and Cintra, some of the largest in Portugal, were shaken from their very foundations, they opened at their summits, and huge masses were flung into the neighbouring valleys. Flames and smoke were emitted from the openings. But much farther away the effects of the great convulsion were experienced. It has been computed, says Humboldt, that a portion of the earth's surface four times greater than the whole extent of Europe was simultaneously shaken. On the coasts of Sweden and on the shores of the Baltic, far away across the Atlantic to the Antigua Islands, at Barbadoes and Martinique, and still further off in the great Canadian Lakes, the movement was sensibly felt. A vast wave of inky blackness swept over the West Indian seas, rising twenty feet above the level of the highest tides. In Algeria the earth was as violently shaken as in Portugal, and eight leagues from Morocco a village with 8,000 inhabitants was swallowed up.

The shocks felt at sea were so violent that captains who experienced them thought their ships had struck the solid ground. A ship 120 miles to the west of St. Vincent was so violently shaken that the men were thrown half a yard perpendicularly upwards from the deck. Lakes and rivers in England were strangely agitated. The water in Loch Lomond suddenly rose against the banks without apparent cause, and then as suddenly subsided—the vibration of the earth's surface

having travelled from Lisbon to Scotland at the rate of twenty miles a minute!

It has been calculated that in Lisbon alone 60,000 persons perished within the brief space of six minutes. But there have been other earthquakes in which even this terrible destruction of life has been surpassed. In 1693, 100,000 persons fell victims to the great Sicilian earthquake, and upwards of 300,000 persons are supposed to have perished in the great earthquakes which desolated Antioch in the sixth and seventh centuries. It has been estimated that within the last 4,000 years five or six millions of human beings have perished through the effects of earthquakes.

It is related that in the great earthquake of 1747 all the inhabitants of the town of Callao were destroyed, save one. The man who escaped, standing on a fort which overlooked the harbour, saw the sea retire to a distance and then return like a vast mountain in height. 'He heard a cry of *Miserere* rise from all parts of the city,' and in a moment all was silent—where the town had once flourished there was a wide sea. But the same wave which overwhelmed the town drove past him a small boat, into which he flung himself, and so was saved.*

No earthquake has ever happened, the circumstances attending which have been so carefully noted as in

* It must be remarked, however, that Sir Charles Lyell estimates the number of the saved at 200, 'of whom twenty-two were saved on a small fragment of the fort of Vera Cruz, which remained as the only memorial of the town after this dreadful inundation.'

the case of the earthquake of Calabria in 1783. This celebrated earthquake began in February, 1783, and lasted until the end of 1786. The first shock threw down, 'in two minutes, nearly every house in all the cities, towns, and villages, from the western flanks of the Apennines in Calabria Ultra to Messina in Sicily, and convulsed the whole country.' The second took place seven weeks later, and was scarcely less violent. Sir Charles Lyell mentions that 'the great granite chain which passes through Calabria from north to south and attains the height of many thousand feet, was shaken but slightly by the first shock, but rudely by those which followed.'

The manner in which a large extent of country was permanently affected by this earthquake is very well worth noticing, as affording an excellent illustration of the mode in which earth-waves travel.

The Apennines are formed for the most part of massive and hard granite, with steep inclines, upon the base of which lie those strata of sand and clay which form the Calabrian plains. These plains are usually level, but are intersected in places by narrow valleys and ravines whose sides are almost vertical. The effect of the earthquake was to *shake down* those parts of the Calabrian plains which border on the granite backbone forming the Apennine range. The soil 'slid over the solid and inclined nucleus, and descended somewhat lower,' says Lyell, 'leaving almost uninterruptedly from St. George to beyond St. Christina—a distance of from nine to ten miles—a chasm between the solid granite

nucleus and the sandy soil. Many lands slipping thus were carried to a considerable distance from their former position, so as entirely to cover others; and disputes arose as to whom the property which had thus shifted its place should belong to.'

The whole of the country over which the effects of the great shocks extended was at times heaved simultaneously, like an angry sea, and sensations resembling sea-sickness were experienced by many of the inhabitants. Those who have watched the sky from the deck of a sea-tossed ship will have noticed that the drifting clouds seem at times to be arrested in their motion: it is in reality the ship which is moving for the moment in the same direction as the clouds, and thus neutralises the effects of their motion. The same phenomenon was observed during the Calabrian earthquake; and nothing serves to give us a stronger impression of the turbulence of those internal heavings which made the dry land as unstable as the billows of a swelling sea. Trees whose roots continued firmly embedded in the soil were seen to lash the ground with their branches.

It will be evident that the seat of disturbance was beneath the rocky ribs of the Apennines. The superincumbent soil was swayed with violence by the vibrating mountain-slopes. But the chief mischief followed when the vibration ceased. For then the soil to which motion had been communicated began to slide over the now stationary granite, and this sliding motion being quickly checked by the irregularities of the rocky

substratum, there resulted a destructive shock to all objects—houses, trees, or living creatures—upon the shaken plains. One may illustrate the nature of the shock as follows:—Suppose a small table-cloth to be lying on a large table with raised edges, and that a variety of objects stand upon the cloth. Then, if the table be shaken with a gradually increasing violence, these objects may continue in safety, provided the motion is so managed that there is no abrupt change of direction, and no sudden increase or diminution of velocity. If the motion of the table be suddenly checked, the cloth would not immediately lose its motion, but would slide till it was stopped by the raised edge of the table; and objects on the cloth would move with it, until its motion was checked, when they would receive a shock more likely to be destructive than any which had been communicated to them while the motion of the table continued. And just as such a cloth would 'rumple up,' as soon as the motion of one end was checked, so the soil of the Calabrian plains was found to be in some parts abnormally raised, in others as strangely depressed. 'In the town of Terranuova,' says Sir Charles Lyell, 'some houses were seen uplifted above the common level, and others adjoining sunk down into the earth. In several streets the soil appeared thrust up, and abutted against the walls of houses: a large circular tower of solid masonry, part of which withstood the general destruction, was divided by a circular rent, and one side was upraised, and the foundations heaved out of the ground.'

As might be expected, the soil did not continue unbroken by the violent shocks to which it was subjected. In the central parts of the disturbed region, the earth opened so widely as to swallow up large houses. In Cannamaria many buildings were 'completely engulphed in one chasm,' insomuch that not a trace of them was ever seen afterwards. So violently did these chasms close their yawning jaws, that afterwards, when excavations were made for the recovery of valuables, the workmen found the contents of houses crushed into a compact mass with detached portions of masonry. In some instances persons were engulphed by one shock and thrown out again alive by the following one.

The magnitude of some of the fissures which were formed during this earthquake affords startling indications of the tremendous violence of the earth's internal throes. Grimaldi observed in the territory of San Fili a newly formed ravine half a mile long and twenty-five feet deep, and another of similar dimensions in Rosarno. In the district of Plaisano three enormous fissures were formed: one a quarter of a mile long, about 30 feet in width, and 225 feet deep; the second, three quarters of a mile long, 150 feet broad, and 100 feet deep; and the third, *nearly a mile long*, 105 feet broad, and 30 feet deep.

If any evidence were required as to the true nature of the disturbance, it would be found in the remarkable motions of masses slightly attached to the surface-soil. Paving-stones were flung into the air, and masses of loose soil flung in showers over the surrounding objects.

In this earthquake 40,000 persons are supposed to have perished, and about 20,000 by the epidemics which followed. Dolomieu gives a painful account of the appearance of the Calabrian cities. 'When I passed over to Calabria,' he writes, 'and first beheld Polistena, the scene of horror almost deprived me of my faculties; my mind was filled with mingled horror and compassion; nothing had escaped; all was levelled with the dust; not a single house or piece of wall remained; on all sides were heaps of stone so destitute of form that they afforded no idea of there having ever been a town on this spot. The stench of the dead bodies still arose from the ruins. I conversed with many persons who had been buried for three, four, or even five days; I questioned them respecting their sensations in so dreadful a situation, and they agreed that, of all the physical evils they endured, thirst was the most intolerable; and that their mental agony was increased by the idea that they were abandoned by their friends, who might have rendered them assistance.'

The destruction of the Prince of Scilla and a great number of his vassals, was one of the most remarkable events attending this deplorable catastrophe. He had persuaded his servants to seek their fishing-boats for safety, and went with them to encourage them. During the night of February 5th, while they were sleeping, an enormous mass of earth was flung from Mount Jaci upon the plain near which the boats were moored. Immediately the sea rose more than twenty feet above

the level of the plain. Every boat was sunk or dashed upon the beach, and hundreds of persons who had been sleeping on the plain were swept out to sea. The Prince and 1,430 of his servants perished.

One of the most remarkable earthquakes ever experienced was that which overthrew Riobamba on February 4th, 1797. A district 120 miles long and 60 broad was shaken by an undulatory motion which lasted for four minutes, and a far wider district felt the effects of the disturbance. Within the space first named, in which the movement was more energetic, every town and village was levelled to the ground; and many places were buried under large masses flung down from the surrounding mountains. Among these was the flourishing town of Riobamba. Preceded and accompanied by no warning noises whatever, the terrific concussion in a few moments effected the complete desolation of the unhappy district. The earthquake was a singular combination of perpendicular, horizontal, and rotatory vibrations. So violent was the perpendicular, or as it may be termed the explosive movement, that hundreds of the wretched inhabitants were flung upon the hill La Culla, several hundred feet high, on the further side of the small river Lican. Then came a horizontal movement, so rapidly succeeding the other that in many instances the furniture of one house was found beneath the ruins of another. In some cases property was removed so far from its original place, that disputes arose among the survivors of the catastrophe, and the Audiencia, or Court of

Justice, was for some time occupied in adjusting these difficulties. Not less remarkable were the effects of circular or rotatory concussions. Walls beyond the town were twisted round without being flung down; rows of trees which had been parallel were deflected in the most remarkable manner; and the direction of the ridges of fields covered with various kinds of grain was observed to be altered by the effects of the earthquake.

Humboldt, it may be mentioned, explains in a somewhat unnatural manner the peculiar effects we have spoken of above. He conceives that the fact of the furniture of one house being found under the ruins of another, seems to show that the movement was first directed downwards, then horizontally, and then upwards. This appears to us wholly improbable. In the first place it has been almost constantly observed that the upward motion (in earthquakes which exhibit perpendicular vibrations) precedes the downward; and secondly, had the downward motion taken place first, it seems most probable that neighbouring houses would have sunk *side by side*, so that the following horizontal movement would only have resulted in the forms of destruction ordinarily observed in earthquakes. The more natural view seems to be that there was first a violent upward movement, flinging the less firmly built houses bodily upwards, and merely destroying others; then immediately followed a downward movement and a horizontal one, bringing the latter class of houses beneath the falling ruins of the others. Or it

may be that so violent was the first upward movement, that the upper parts of all buildings were flung into the air, whence—not partaking in the horizontal movement which displaced the foundations and lower part of the houses—they fell in ruins over the *débris* of buildings they had not belonged to originally. An upward, followed by a downward, and then by a horizontal movement, might result in either form of demolition, or in both.

A short time after the destruction of Riobamba, a fearful subterranean rumbling, resembling the loudest thunder-peals, was heard under the cities of Quito and Ibarra, the former more than a hundred miles from Riobamba.

The subterranean noises heard during earthquakes are sometimes singularly striking. The nature of the noises is very various, says Humboldt, 'rolling, rattling, clanking like chains, occasionally like thunder close at hand; or it is clear and ringing, as if masses of obsidian or other vitrified matters were struck in caverns underground.' These noises are not only heard much farther off than they could be if they were transmitted in the air, but they travel much more rapidly. In 1744, when the great eruption of Cotopaxi took place, subterranean noises were heard at Honda, on the Rio Maddalena. The crater of Cotopaxi, 17,000 feet above the level of Quito, is separated from it 'by the colossal mountain-masses of Quito, Pasto, and Papayan, by innumerable valleys and precipices, and by an actual distance of no less than 500 geographical

miles.' The eruption which took place in the island of St. Vincent on the 30th of April, 1812, produced subterranean noises resembling the loudest peals of thunder in Caraccas, in the plains of Calabozo, and on the banks of the Rio Apure, a distance of upwards of 700 geographical miles. 'This, in respect of distance,' says Humboldt, 'was as if an eruption of Mount Vesuvius were to be heard in the north of France.'

But it is remarkable that subterranean rumblings and bellowings are sometimes heard when neither an earthquake nor the kindred phenomenon—a volcano—is in progress. 'Sonorous phenomena,' Humboldt tells us, 'when accompanied by no perceptible shocks, leave a remarkably deep impression even with those who have long dwelt in districts subject to repeated earthquakes.' A singular instance occurred in the year 1784, in the high lands of Mexico. A sound was heard as of heavily rumbling thunder alternating with sharp explosive bursts beneath the feet of the startled inhabitants of Guanaxato. The subterranean bellowings and thunderings (*bramidos y truenos subterraneos*) grew gradually more and more intense, and then decreased as gradually. Terrified by a phenomenon which seemed to forewarn them of an approaching and terrible catastrophe, the inhabitants fled from the town, leaving great piles of silver bars a prey to bands of robbers. But after a time the more courageous returned and repossessed themselves of their treasure. For one month the subterranean grumblings were heard at intervals, though neither on the surface of the earth,

nor in the silver mines 500 yards beneath it, was any movement of the earth perceptible.

Earthquakes occur in all regions adjacent to active volcanoes. Thus the neighbourhood of Vesuvius, Etna, and Teneriffe is infested by subterranean convulsions, which also are frequent over the neighbourhood of the Greek Archipelago, and in Syria. In fact it seems probable that the whole of the Mediterranean basin and the surrounding lands for a distance of many miles from its shores form a single earthquake district, whereof Teneriffe, Vesuvius, Etna, Stromboli, the Archipelagic and the Syrian volcanoes are the safety-valves. Then there is another earthquake region surrounding Hecla or—some say—extending in a long line from the Jan Mayen volcano, through Hecla, the Azores, and the Cape Verde Islands, to St. Helena and Tristan d'Acunha. Japan, Sumatra, Java, and the Islands of the East Indian Archipelago are liable to fearful earthquakes,—some of the most destructive of which have occurred within the past few years. In the West Indies there is another earthquake region, to which must be referred those which have recently taken place. Probably this district belongs to the great earthquake region in Columbia and Peru, around the celebrated volcanoes Cotopaxi and Chimborazo. The south-western district of the United States are also liable to earthquake shocks, apparently referable to the great Mexican volcanoes. There is one region of the earth in which subterranean shocks occur which cannot be referred to the neighbourhood of volcanic vents. Upper India and parts of Western India are liable to frequent earthquakes,

insomuch that between the years 1800 and 1842 no less than 162 earthquakes were recorded in these places. Undoubtedly we may trace these disturbances to the great mountain chains which traverse this part of Asia. The subterranean forces which upheaved the great Himalayan range, for instance, may be assumed to be still existent, though now for a while dormant, or 'perhaps,' says Sir John Herschel, 'expended in maintaining the Himalayas at their present elevation.'

On the other hand there are some regions wholly free from earthquake shocks. Among such may be mentioned the great alluvial plains of America east of the Andes, the plains on the north-east of Europe, and the northern parts of Asia. There are monuments, natural and artificial, which prove the absolute fixity of some regions. The slightest shock would have flung down that strange mass which is perched upon the summit of the Peter Botte mountain, 1,500 feet above the sea-level. Pompey's Pillar justifies the assertion of Strabo that Egypt has long been free from earthquakes; though nothing short of subterranean convulsion could have flung down the more ancient obelisks which lie prostrate amidst the sands of Western Lower Egypt. Even that masterpiece of Egyptian labour, the Great Pyramid, though surpassing all other human erections in stability, shows unmistakable evidence of the slow action of subterranean forces.* In Mexico, again, in the very centre seemingly, of earth-

* 'The quantity of the post-pyramid *tilt*,' says Professor Piazzi Smyth, 'appears to be about thirty-seven seconds,' as given by the corner angles of the Great Pyramid.

rocking forces, there is a region in which rocks of grotesque figure attest the perfect immunity which the region has enjoyed even from inconsiderable shocks. The Cheese-ring in Devonshire is another instance of the kind of evidence we are considering.

And as there are instances of regions near to a disturbed district which yet are free from shocks, so there are spots liable to frequent shocks though the neighbouring country for miles on every side is seldom (if ever) disturbed. Such is the district—very limited in extent—near Comrie, in Perth, where a year scarcely ever passes without a shock being experienced.

It would seem also as if regions free from subterranean disturbance for many centuries must not count upon permanent immunity. For a violent earthquake will often open out, as it were, a passage for subterranean impulses to new regions. 'The circles of concussion enlarge,' says Humboldt, 'in consequence of a single extremely violent shock.' Since Cumana was destroyed (December 14, 1797) every shock of the southern coast is felt in the peninsula of Maniguarez, which before suffered no disturbance. Again, in the successive earthquakes which traversed (in 1811–13) the valley of the Mississippi, Arkansas and Ohio rivers, it was noteworthy how the motion travelled farther and farther northward on each occasion. It seemed as if the subterranean forces were gradually breaking a way through successive barriers.

We have seen so much of the earthquake as an agent of destruction, that it may sound paradoxical to assert

that the phenomenon is surpassed by no other as a regenerative and restorative agent. Yet this is strictly the case. But for earthquakes our continents would continually — however slowly — diminish in extent through the action of the sea-waves upon their borders, and of rain and rivers on their interior surfaces. 'Had the primeval world been constructed as it now exists,' says Sir John Herschel, 'time enough has elapsed, and force enough, directed to that end, has been in activity, to have long ago destroyed every vestige of land.' It is to the reproductive energy of the earth's internal forces that we are alone indebted for the very existence of dry land. To the same cause, undoubtedly, we owe that gradual process of change in the configuration of continents and oceans which has been for ages and still is in progress—a process the benefit derived from which cannot possibly be called in question. Our forests and our fields derive their nourishment from soils prepared, for long ages, beneath the waves of ocean; our stores of coal and of many other important minerals have been in like manner prepared for our use during the long intervals of their submergence; we build our houses, even, with materials many of which owe their perfect adaptation to our wants to the manner in which they have been slowly deposited on what was once the bed of ocean, and compressed to a due solidity and firmness of texture beneath its depths. If it is indeed true, as Humboldt asserts, that 'the destiny of man is in part dependent on the fashion of the outer crust of the globe, on the partitioning of

continents, on the direction of the mountain chains which traverse them, and on the distribution of land and water,' then we must look upon the earthquake as the most important of all those agencies which tend to the renovation of our terrestrial globe. So far from dreading lest the earth's subterranean forces should acquire new energies, we ought rather to fear lest they should lose their force. We may, therefore, gladly hail the opinion of the great geologist who asserts that 'the energy of subterranean movements has always been uniform as regards the *whole* earth. The force of earthquakes,' adds Lyell, 'may for a cycle of years have been invariably confined, as it is now, to large but determinate spaces;' gradually, however, this force shifts in position, so that other regions, for ages at rest, become 'in their turn the grand theatre of action.'

(From the *Cornhill Magazine* for July 1868.)

THE ANTARCTIC REGIONS.

There are parts of our earth of which we know less than of the moon, or even of some of the planets. The eyes of the astronomer have looked upon the unattainable summits of the lunar mountains; he has studied the arid wastes which lie within the lunar craters; he has measured the light which these regions reflect—nay, even the degree to which they are warmed under the blazing sun of the long lunar day. Passing

beyond the moon, the astronomer has studied the lands and seas of a world which has justly been termed a miniature of our earth: he has watched the clouds which form over the continents and oceans of the planet Mars, and are dissipated even like our own by the solar rays; he has determined the very constituents of that planet's atmosphere. But more than this, the astronomer has actually studied the condition of parts of Mars, where (if analogy can be trusted) the very inhabitants of that world are unable to penetrate. The ruddy orb (which when these lines appear will be shining conspicuously in our skies after a long absence from the earth's neighbourhood) presents to the astronomer its Arctic and Antarctic wastes. He is able to watch the gradual increase of either region as winter prevails alternately over the northern and southern hemisphere of Mars; he can measure their gradual reduction with the progress of the Martial summer: and he can infer from their aspect that even in the height of summer there still remain ice-covered regions so wide in their range as doubtless to defy the efforts of the Martialists to penetrate to the poles of the globe on which they live. So that where most probably no living creature on Mars has ever penetrated the astronomer can direct his survey; and questions which no Martial geographer can pretend to answer the terrestrial astronomer can discuss with a considerable degree of confidence. It is the same even with the more distant planets Jupiter and Saturn. Despite the vast spaces which separate us from these orbs, we yet know much

respecting their physical habitudes; and whereas our knowledge of our own earth is limited by certain barriers as yet unpassed, and probably impassable, there is no part of the surface of either of the giant planets which has not come under the astronomer's scrutiny.

These considerations suggest in turn the strange thought that possibly the unattained places of our earth have been viewed by beings which are not of this world. I say *possibly*, but I might almost say *probably*. It seems in no degree unreasonable to suppose not merely that the earth's sister-planet Venus is inhabited, but that some creatures on Venus possess the reasoning powers and the insight into the secrets of Nature which have enabled the inhabitants of Earth to study the orbs which circle like herself around the sun. If this be the case—if there are telescopists in Venus as skilful as those inhabiting our earth—they are able to answer questions which hitherto have baffled our geographers. They may not, indeed, have the means of ascertaining details respecting the structure of our continents and oceans. They cannot know, for instance, whether the region to which Livingstone has penetrated is, as he supposes, the head of the river we terrestrials call the Nile, or, as others suppose, is in reality the head of the Congo. For certainly no telescopic powers possessed by our astronomers could give us information on such points, if our position were interchanged with that of the inhabitants of Venus. But astronomers in Venus can,

without excessive telescopic power, inform themselves whether our polar regions are like the corresponding regions in Mars—or whether, as many geographers suppose, the Arctic regions are occupied in summer by an open ocean, while in the Antarctic regions there is a large continent.

A new interest has recently been given to inquiries respecting the condition of Arctic and Antarctic regions, by the circumstance that the expedition of the 'Challenger' is expected to bring us information respecting the latter regions, while application has been made for Government assistance towards an Arctic expedition. I propose to consider, now, some of the questions which are connected with Antarctic research, and in particular to discuss the probability of the existence of great continental lands within the Antarctic circle.

Before proceeding to consider these points, however, I have a few remarks to make on the question of Government aid to this branch of geographical research.

It should be remembered by those who discuss this subject that the first explorations of the polar regions of our earth had a commercial origin. It was supposed that by finding a passage round the northern shores of the American continent, communication with China and the East Indies would be facilitated. A way had been found round Cape Horn, but the way was long, and the storms which rage in Antarctic seas rendered the route uninviting to the contemporaries of Magel-

lan. The natural supposition in those days was, that voyagers from the great maritime northern countries—from England, from Spain and Portugal, or from the Netherlands, would find their advantage in sailing northwards rather than southwards. Hence the long and persistent efforts made to discover a north-western passage. Nor were the more directly Arctic voyages of Hudson and Richardson conducted with any other primary purpose. It is indeed manifest, as any one will perceive on examining a terrestrial globe, that a north-eastern course would avail nearly as well as a north-western for reaching eastern countries from Europe; and that a directly polar course would be better than either—if only (as Hudson hoped) a safe passage might be found through the Arctic seas.

Gradually, as the hope of finding a north-western passage available for commerce died out, other circumstances encouraged persistence in the efforts which had been made to penetrate the regions lying to the north of the American continent. There was much, indeed, in the desire to accomplish what had foiled so many; and it may be questioned whether this desire had not a good deal to do with the appeals which were made for Government assistance, as also with the ready response of Government to those appeals. Nevertheless, a real scientific interest had become associated with the search after a north-west passage. The magnetic pole of the earth was known to lie somewhere amid the dreary archipelago, with its ice-bound inlets, and glacier-laden shores, through which our Arctic seamen had so

long attempted to penetrate. There, also, lies one of the northern poles of cold; while the configuration of the isothermal lines (or lines of equal temperature) in the neighbourhood, shows how some influence is at work carrying relative warmth from the Atlantic towards the North Pole, and leaving the regions on the west of that course exposed to a degree of cold greatly more intense. To these considerations, others connected with the whaling trade were added, though I am not prepared to say that (so far as the question of Government assistance was concerned) these considerations had very great weight.

It cannot be denied, however, that at a certain stage in the history of Arctic voyaging, the mere barren ambition to attain or approach the North Pole of the earth, was set in advance of more practical considerations. We find, for instance, that in the case of Parry's 'boat and sledge' expedition from Spitzbergen polewards, certain sums of money were set as a reward for reaching such and such northern latitudes, the sum of ten thousand pounds being the prize for attaining the North Pole itself.

It appears to me that those have done well who, during their recent discussion of the subject, have laid stress upon the scientific value of the results which may be obtained during successful Arctic and Antarctic voyages. It is unworthy of a great country to appeal to the national honour on a matter so insignificant as the actual approach which has been made to either pole of the earth—to reason that because England has

been thus far fortunate, so that sons of hers have made the nearest approach as well to the Arctic as to the Antarctic pole, and because Germany, Sweden, and the United States seem likely to send their ships as near or nearer to either pole, *therefore* England should send out an expedition to forestall the seamen of those countries. A better reason should be given for expeditions into the dangerous polar regions; and such a reason has been found, we think, in the scientific interest and value of such voyages.*

* I would venture, however, to speak somewhat earnestly in opposition to the attempt which has been made to attach meteorological importance to polar voyages in connection with solar observations. A persistent effort has recently been made to show that, by the study of the sun, an answer may be given to the long-vexed question whether the weather can be predicted; and assertions have been very confidently made as to successes already achieved in this inquiry. It cannot be too strongly insisted that there is nothing to encourage the hope of such success, or rather, that there is every reason to feel assured that no success can be obtained. It has been shown, indeed, that in a certain subtle way, and by no means to an important degree, rainfall is associated with the great cycle of solar spot changes. It has also been shown that probably the hurricanes of tropical regions are somewhat more numerous during the periods of great solar disturbance than at other times. Moreover, terrestrial magnetical disturbances are connected with solar disturbances, and are known to be more numerous during periods of sun-spot frequency than at other times. That a connection should thus have been traced between terrestrial phenomena and the most marked of all the cyclic changes affecting the sun's surface is not surprising. But so far is the circumstance from encouraging the hope to which I have referred, that it is altogether discouraging, and indeed seems to negative absolutely all hopes of success in forming any weather presages from the study of the sun's surface. For be it noticed, that not one of these effects gives us any absolute information as to the weather, either as respects rainfall, wind, or magnetical phenomena. We only know that probably there will be more or less rainfall with certain winds, a greater average annual number of hurricanes, and an excess of magnetical activity on the whole. Such information is all but

This remark might have been applied with special force to Antarctic voyages if an attempt had been made, somewhat earlier, to penetrate to regions where Antarctic observing stations might have been established for watching the transit of Venus in December 1874. This important astronomical event could have been observed with great advantage from the Antarctic regions. It is easy to show why this is the case. Regarding our earth as a globe-shaped house, whence observations can be made as from different rooms, we see that in December, when the south polar regions are enjoying their summer—or, in other words, are turned sunwards—the Antarctic regions are very suitable *lower rooms*, as it were, for observing Venus crossing the sun. It is as seen from these lower regions, that she will seem to traverse the sun along the highest course. Now the determination of the sun's distance, by observations of Venus in transit, depends wholly on getting (i.) as *high* a view, and (ii.) as *low* a view of the planet as possible, and noting the different effects thus perceived. Astronomers are going as far north as they can—indeed, they

valueless, and yet it is all we have obtained from the most striking of solar phenomena. How utterly hopeless, then, must it be to expect results of value from the study of solar details relatively quite insignificant. I venture to speak strongly on this point. It is known that Government has been singularly liberal in affording aid to researches promising results of meteorological importance. Ten thousand pounds per annum have long been paid for observations based on hopes of the sort, and, in the opinion of those best qualified to judge, the results have had no scientific value whatever. Neither our men of science nor our Government can well afford to repeat the experiment where the chances of success are even more hopelessly chimerical.

are going to stations which, as seen from the sun at the time, would seem to be at the very top of our terrestrial house—but they are not going to occupy the lowest rooms. They will go no nearer than Kerguelen Land—if so near; for, by an unfortunate mistake, it was announced several years ago that in 1874 it would be useless, owing to certain effects depending on the earth's rotation, to visit any Antarctic stations; and, as a matter of fact, Antarctic voyages were deferred until the approach of the transit of 1882, when it was supposed that the circumstances would be more suitable. Three years ago geographers and Arctic seamen were invited to prepare for voyages in anticipation of the latter transit (for it will be understood that several years are required for suitable preparations), when, to the astonishment of the astronomical world, it was discovered, that whereas observations at Antarctic stations in 1874 would have been highly advantageous, such observations in 1882 would scarcely have the slightest chance of success. The preparations, therefore, for observing the latter transit were countermanded; but though the discovery came in good time to save England from the discredit of undertaking dangerous expeditions on the strength of erroneous calculations, it was too late for utilising Antarctic stations during the transit of 1874.

Nevertheless a considerable amount of scientific interest attaches to Antarctic exploration, especially since it has been decided that a Government expedition shall devote some of its energies to researches upon the

borders of the Antarctic regions. The general instructions to this effect are contained in the following passage from the Report of the Circumnavigation Committee of the Royal Society: 'It is recommended . . . . to pass . . . . across the South Atlantic to the Cape of Good Hope; thence by the Marian Islands, the Crozets, and Kerguelen Land, to Australia and New Zealand, going southwards, *en route*, opposite the centre of the Indian Ocean, as near as may be with convenience and safety to the southern ice-barrier. . . . This route will give an opportunity of examining . . . . the specially interesting fauna of the Antarctic seas. Special attention should be paid to the botany and zoology of the Marian Islands, the Crozets, Kerguelen Land, and new groups of islands which may possibly be met with in the region to the south-east of the Cape of Good Hope. Probably investigations in these latitudes may be difficult: it must be remembered, however, that the marine fauna of these regions is nearly unknown; that it must bear an interesting relation to the fauna of high northern latitudes; that the region is inaccessible, except under such circumstances as the present; and that every addition to our knowledge of it will be of value.' We find, also, among the suggested physical observations, the remark that 'it is in the Southern Ocean that the study of ocean temperatures, at different depths, is expected to afford the most important results, and it should there be systematically prosecuted. The great ice-barrier should be approached as nearly as may be deemed suitable, in a meridian nearly corres-

ponding to the centre of one of the three great southern oceans—say to the south of Kerguelen Land—and a line of soundings should be carried north and south as nearly as may be.' And it need hardly be said that observations of meteorological and magnetic phenomena in the southern seas will not be neglected.

It will be seen that direct Antarctic exploration will not be attempted. No effort will be made to penetrate within the ice-barrier to which these instructions refer as to some line of demarcation separating the known from the unknown. Nor would it be easy, perhaps, to assign any sufficient reason for the renewal, by a scientific expedition, of those arduous explorations in which Wilkes, d'Urville, and (especially) the younger Ross, discovered all that is known about the Antarctic ice-barrier. There was much, indeed, in the results obtained by Ross to invite curiosity on the one hand, and on the other to show that the Antarctic regions can be penetrated successfully in certain directions. It seems far from unlikely that other openings exist by which the southern pole may be approached, than that great bay, girt round by steep and lofty rocks, where Ross made his nearest approach to the southern magnetic pole. I shall presently indicate reasons for believing that the Antarctic as well as the Arctic regions are occupied by an archipelago—ice-bound, indeed, during the greater part of the year—but nevertheless not altogether impenetrable during the Antarctic summer. Yet there is little to encourage any attempts to explore this region otherwise than in ships specially constructed to encounter its dangers.

It is singular how confidently geographers have spoken of the great Antarctic continent, when we remember that only an inconsiderable extent of coast line has even been seen by Antarctic voyagers in any longitudes, except where Ross made his nearest approach to the South Pole. There is absolutely not a particle of evidence for believing that the ice-barriers which have been encountered—Sabine Land, Adélie Land, Victoria Land, and Graham Land — belong to one and the same land region. It is not, indeed, certain that all the mapped coast-line is correct—for it must not be forgotten that where Commodore Wilkes charted down a coast line Ross found an open (or only ice-encumbered) sea, *and sailed there.*

Yet Dr. Jilek, in the *Text-book of Oceanography*, in use in the Imperial Naval Academy of Vienna, writes thus confidently respecting the Antarctic continent: 'There is now no doubt,' he says, 'that around the South Pole there is extended a great continent, mainly within the polar circle, since, although we do not know it in its full extent, yet the portions with which we have become acquainted, and the investigations made, furnish sufficient evidence to infer the existence of such with certainty. This southern or Antarctic continent advances farthest in a peninsula S.S.E. of the southern end of America, reaching in Trinity Land almost to 62 degrees south latitude. Outwardly these lands exhibit a naked, rocky, partly volcanic desert, with high rocks destitute of vegetation, always covered with ice and snow, and so surrounded with ice that it is

difficult or impossible to examine the coast very closely.'

A singular, and indeed fallacious, argument has been advanced by Capt. Maury in favour of the theory that the Antarctic regions are occupied by a great continent. 'It seems to be a physical necessity,' he argues, 'that land should not be antipodal to land. Except a small portion of South America and Asia, land is always opposite to water. Mr. Gardner has called attention to the fact that only one twenty-seventh part of the land is antipodal to land. The belief is, that on the polar side of 70 degrees north we have mostly water, not land. This law of distribution, so far as it applies, is in favour of land in the opposite zone.' Surely a weaker argument has seldom been advanced on any subject of scientific speculation. Here is the syllogism: we have reason to believe (though we are by no means sure) that the Arctic regions are occupied by water; land is very seldom found to be antipodal to land; therefore, probably, the Antarctic regions are occupied by land. But it is manifest that, apart from the weakness of the first premiss, the second has no bearing whatever on the subject at issue, *if the first be admitted*: for we have no observed fact tending to show that water is very seldom antipodal to water, which would be the sole law to guide us in forming an opinion as to the regions antipodal to the supposed Arctic water. On the contrary, we know that water is very commonly antipodal to water. We have only to combine what is known respecting the relative proportions of land and

water on our globe with Mr. Gardner's statement that twenty-six out of twenty-seven parts of the land are antipodal to water, to see that this must be so. There are about 51 millions of square miles of land and about 146 millions of square miles of ocean. Now about 49 millions of square miles of land are antipodal to water, accounting therefore for only 49 millions out of the 146 millions of square miles of ocean surface; the remaining 97 millions of square miles of ocean are, therefore, not antipodal to land, but one half (any we please) antipodal to the other half. In fact, we have this rather singular result, that the ocean surface of the globe can be divided into three nearly equal parts, of which one is antipodal to land while the other two parts are antipodal to each other. This obviously does not force upon us the conclusion that an unknown region must be land because a known region opposite to it is oceanic; and still less can such a conclusion be insisted upon when the region opposite the unknown one is itself unknown.*

* Whether the relation above-mentioned respecting land regions is noteworthy may very well be questioned. It will be seen that Capt. Maury regards it as seemingly a physical *law* 'that land should not be antipodal to land.' Now this is by no means satisfactorily indicated. As a question of probability it is not certain that the present relation, by which twenty-six parts out of twenty-seven of the land are antipodal to water, can be regarded as antecedently an unlikely one, when nearly three-fourths of the whole surface are occupied by water, and when, also, the bulk of the land and water regions consist of such great surfaces as those we call continents and oceans. Granted these preliminary conditions, it would appear, indeed, that only by a very remarkable and, as it were, artificial arrangement of land and water could any but a small proportion of the land be antipodal to land. The

So far, indeed, as the geographical evidence extends, it seems probable that there exists within the Antarctic circle an elevated region bearing somewhat the same relation to the great promontories terminated by Cape Horn and the Cape of Good Hope, as well as to the relatively elevated region indicated by the islands south and south-east of Australia, which the Hindoo Koosh bears to the great mountain-ranges of Asia. We seem to have in the Antarctic high lands the great central elevation whence three great lines of elevation extend. That the great mountain range which forms the backbone of South America is continued under water, rising again in the South Shetland Isles and Graham's Land, would indeed seem altogether probable; and it may be remarked as a coincidence of some importance that the mountains seen by Ross on the other side of the Antarctic Circle—Mounts Sabine, Crozier, Erebus, and Ross—lie in a chain tending in the same direction. But although we might thus be led to regard the Antarctic regions as forming a great central region of elevation, it by no means follows that this region is of the nature of a table land.

Meteorological considerations have been urged by Maury for the theory of Antarctic lands in large masses, 'relieved by high mountains and lofty peaks.' He considers that it is to such mountains (performing the

stress laid by Maury on the observed relation seems to me, indeed, as unwarranted as that laid by Humboldt on the fact that the great southerly projections of the land lie nearly in the same longitude as the great northerly projections.

part of condensers) that the steady flow of 'brave' winds towards the South Pole is to be ascribed. 'Mountain masses,' he says, 'appear to perform in the chambers of the upper air the office which the jet of cold water discharges for the exhausted steam in the condenser of an engine. The presence of land, therefore, not water, about this south polar stopping-place is suggested.' And he attaches considerable weight, in this connection, to the circumstance that the barometric pressure is singularly low over the whole Antarctic Ocean,*—as though there were here the vortex of a mighty but steady whirlwind. 'We may contemplate the whole system of "brave west winds," circulating in the Antarctic regions, in the light of an everlasting cyclone on a gigantic scale—the Antarctic continent in its vortex—about which the wind in the great atmospherical ocean all round the world, from the pole to the edge of the calm belt of Capricorn, is revolving in spiral curves, continually going with the hands of a watch, and twisting from right to left.' However, it would be unsafe to base the theory of an Antarctic continent on speculations such as these. And still less

* This curious circumstance cannot be explained, as Maury supposes, by the existence of upflowing currents of air, however occasioned. The total pressure of the air over any region is not affected by motions taking place within the air, any more than the total pressure of water upon the bottom of a tank is affected by motions taking place in the water. There are reasons for believing that the true explanation of the low Antarctic barometer lies in the fact that the ocean surface is in Antarctic regions *above*, and in Arctic regions *below*, the mean level. The excess of ocean surface in the southern hemisphere indicates an overflow, as it were, of water southwards, which must lead to such a relation.—See my 'Light Science for Leisure Hours,' Second Series.

can we assume with Maury that Antarctic volcanoes play an important part in the economy of southern meteorological phenomena. There is no reason for supposing that active volcanoes have any special action in determining atmospheric relations. Capt. Maury suggests that we may, 'without transcending the limits of legitimate speculation, invest the unexplored Antarctic land with numerous and active volcanoes,' and this certainly may be granted, for two volcanoes (one in action) have been seen there. But it would be unsafe to infer that such volcanoes are 'sources of dynamical force sufficient to give that freshness and vigour to the atmospherical circulations which observations have abundantly shown to be peculiar to the southern hemisphere.' Volcanoes would need to be so numerous and so active, in order to produce the imagined effect, that the whole southern continent would be aglow like a gigantic furnace. A hundred Etnas would not produce the thousandth part of the in-draught which Maury ascribes to Antarctic volcanoes. Assuredly, we may say with Maury, but more significantly, that 'volcanoes are not a meteorological necessity.' 'We cannot say that they are,' he proceeds, 'yet the force and regularity of the winds remind us that their presence there would not be inconsistent with known laws.' He believes, in fact, that the steady winds may be partly formed as an in-draught feeding volcanic fires. It is as well to remember, when ideas so wild are mooted, that, as Maury himself remarks, 'we know, ocularly, but little more of the topographical features of Antarctic regions

than we do of those of one of the planets.' 'If they be continental,' as he proceeds, 'we may indeed, without any unwarrantable stretch of the imagination, relieve the face of nature there with snow-clad mountains, and diversify the landscape with flaming volcanoes;' but we must not forget that this *is* a work of imagination, not a theory which can be insisted upon as though it represented a geographical fact.

While on this subject, however, we cannot refrain from quoting a very striking passage from a letter by Capt. Howes, of the 'Southern Cross,' because, although it relates in reality to the phenomena of an Aurora Australis, it presents a scene such as we might conceive to accord with the conception of an Antarctic region covered with volcanoes whose combined action made the whole continent at times as one vast furnace. Apart from fancies such as these, the description is full of interest:—'At about half-past one,' he says, 'on the second of last September, the rare phenomenon of the Aurora Australis manifested itself in a most magnificent manner. Our ship was off Cape Horn, in a violent gale, plunging furiously into a heavy sea, flooding her decks, and sometimes burying her whole bows beneath the waves. The heavens were as black as death; not a star was to be seen when the brilliant spectacle first appeared. I cannot describe the awful grandeur of the scene; the heavens gradually changed from murky blackness till they became like livid fire, reflecting a lurid, glowing brilliancy over everything. The ocean appeared like a sea of vermilion lashed into fury by

the storm; the waves, dashing furiously over our side, ever and anon rushed to leeward in crimson torrents. Our whole ship—sails, spars, and all—seemed to partake of the same ruddy hues. They were as if lighted up by some terrible conflagration. Taking all together, the howling, shrieking storm, the noble ship plunging fearlessly beneath the crimson-crested waves, the furious squalls of hail, snow, and sleet driving over the vessel and falling to leeward in ruddy showers, the mysterious balls of electric fire resting on our mast-heads, yard-arms, &c., and above all the awful sublimity of the heavens, through which coruscations of auroral light would often shoot in spiral streaks and with meteoric brilliancy, altogether presented a scene of grandeur and sublimity surpassing the wildest dreams of fancy.'

The enormous icebergs which come from out the Antarctic seas suggest interesting conclusions respecting regions as yet unexplored. This will be understood when it is remembered that all the larger and loftier icebergs have in reality had their origin in vast glaciers. Vast masses of ice are formed, indeed, in the open sea. Each winter the seas which have been open during the summer months (December, January, and February) are covered over with ice of enormous thickness, and when summer returns the ice-fields thus formed are broken up, and the fragments, borne against each other during storms, become piled into gigantic masses. But the agglomerations thus formed, vast though they are, are far exceeded in magnitude by the true icebergs. 'Among the drifting masses of flat sea-ice,' says Tyn-

dall, 'vaster masses sail which spring from a totally different source. These are the icebergs of the polar seas. They rise sometimes to an elevation of hundreds of feet above the water, while the height of ice submerged is about seven times that seen above.' 'What is their origin?' he proceeds, speaking of those met with in the northern seas. 'The Arctic glaciers. From the mountains in the interior the indurated snows slide into the valleys, and fill them with ice. The glaciers thus formed move, like the Swiss ones, incessantly downwards. But the Arctic glaciers reach the sea, and enter it, often ploughing up its bottom into submarine *moraines*. Undermined by the lapping of the waves, and unable to resist the strain imposed by their own weight, they break across, and discharge vast masses into the ocean. Some of these run aground on the adjacent shores, and often maintain themselves for years. Others escape, to be finally dissolved in the warm waters of the ocean.'

Now, it is important to notice that the Antarctic icebergs are vaster and more numerous than those formed in Arctic seas. How large these last are will be understood from the instance referred to by Tyndall, who, citing Sir Leopold McClintock, describes an Arctic iceberg 250 feet high, and aground in 500 feet of water. But Captain Maury speaks of Antarctic icebergs in the open sea, hundreds of feet high and 'miles in extent.' 'The belt of ocean that encircles this globe on the polar side of fifty-five degrees south latitude is never free from icebergs,' he adds; 'they are formed in all

parts of it all the year round. I have encountered them myself as high as the parallel of thirty-seven degrees, . . . and navigators on the voyage from the Cape of Good Hope to Melbourne, and from Melbourne to Cape Horn, scarcely ever venture, except while passing Cape Horn, to go on the polar side of fifty-five degrees.' As he justly remarks, 'the nursery for the bergs to fill such a field must be an immense one; such a nursery cannot be on the sea, for icebergs require to be fastened firmly to the shore until they attain full size. They, therefore, in their mute way, are loud with evidence in favour of Antarctic shore-lines of great extent, of deep bays where they may be formed, and of lofty cliffs whence they may be launched.'

It is remarkable, however, that Maury fails to notice that the evidence of these enormous icebergs is opposed to the theory of an Antarctic continent, or is, at least, by no means in favour of that theory. It might at once be objected, indeed, to the inferences derived by Maury from the Antarctic icebergs, that similar reasoning would show the unknown parts of the Arctic regions to be mainly occupied by land masses. But, apart from this, all that we know of glaciers teaches us to recognise the fact that they are formed only in regions where vast mountain ranges exist, and where the lower levels are reached by ravines and valleys gradually diminishing in slope as they descend. Now, wherever this is the contour of the land, we have in the surrounding regions one or other of the three following conditions:—either (i.), flat land regions

around the base of the mountain ranges; or (ii.), inland seas upon which the valleys debouch; or (iii., and lastly), open sea, in which the mountain ranges form islands or pinnacles complicated in figure. It is clear that only the third of these formations corresponds to the conditions indicated by the Antarctic icebergs. There must be a communication between Antarctic seas and the mountain-slopes of Antarctic lands, and this communication must be by long and deep valleys, descending to fiords, bays, and gulfs. It is thus as certain as such a matter can be until the eye of man has actually rested on these regions, that the Antarctic shores are extremely irregular; and it seems altogether more probable that the land-masses of Antarctic regions consist of a number of large islands like those in the seas to the north of America, than that there is a great continental region, broken along its border, like the Scandinavian peninsula, into bays and fiords.

But strangely enough, Captain Maury actually recognises the necessity for a suitable region within which the icebergs are to be formed, but seems to feel bound (by the opinion of geographers respecting the unknown Antarctic regions) to reconcile the existence of such a region with the theory of a great Antarctic continent. 'Fiords, deep bays, and capacious gulfs loom up,' he tells us, 'before the imagination, reminding us to ask the question, Is there not embosomed in the Antarctic continent a Mediterranean, the shores of which are favourable to the growth and

the launching of icebergs of tremendous size? and is not the entrance to this sea near the meridian of Cape Horn, perhaps to the west of it?' But the condition of the Antarctic seas will not permit us to adopt such a view of the origin of southern icebergs. Even if the imagined Antarctic Mediterranean were not icebound, it would be sufficiently difficult to conceive that the glaciers formed around its shores would pass out in stately procession through the imagined straits south and west of Cape Horn. How should currents sufficiently strong be generated to bear these glacial masses away? How could collisions, blocking up the mouth of the strait often for months together, be avoided? And when the consideration is added that an Antarctic Mediterranean would almost certainly be frozen over, the whole year through, the theory that it is within such a sea that Antarctic glaciers are formed becomes, in our opinion, altogether untenable. If such a sea exists, it must be blocked up with ice too completely for any considerable movements to take place within it. Even the glaciers on its borders must be unlike the glaciers known to us, because the downward motion of the ice-masses composing them must be so checked by the resistance of masses already accumulated, as to be scarcely perceptible even in long periods of time.

If we consider the nature of the Antarctic seas, and particularly the circumstance that the Antarctic summer is far colder than the Arctic summer, it will appear most probable that within the Antarctic regions

land and water are so distributed that, while the shore-lines are of great extent, there is very free communication with the open Antarctic Ocean. In other words, it seems reasonable to conclude that there are many large islands within the Antarctic circle, that these islands are separated from each other by wide passages, and not by straits readily blocked up and encumbered with ice in such sort as to impede the outward passage of the great icebergs. And nothing which has been ascertained by Antarctic voyagers is opposed to this conclusion. It is indeed very easy to fall into the mistake of inferring otherwise from the study of an ordinary chart of the Antarctic seas. If, for example, we look at the chart in Maury's *Physical Geography of the Sea*, we are apt to imagine that the boundary-line indicating the limits of Antarctic explorations, points to the existence of a continuous barrier of ice, the advanced line of defence, as it were, behind which lies as continuous a barrier of precipitous shore-line. But a very slight study of the records of Antarctic voyages will suffice to show how erroneous is such an impression. We find that long before coast-lines have been seen, the hardy voyagers have found themselves impeded and often surrounded by masses of floating ice. Wilkes, Ross, and d'Urville, when struggling to advance towards the southern pole, were repeatedly compelled to retreat without seeing any signs of land. Land has not been seen, indeed, along more than one-sixth part of the circuit of the Antarctic barrier, and it has only been in the neighbourhood of

Victoria Land that a continuous coast-line of any considerable extent has been discovered. Wherever land has been seen, it has been mountainous and rugged—a circumstance which suggests great irregularity of outline in the land-regions, and the high probability that these regions are broken up into islands resembling those in the north-polar seas.

Certainly, there is much in what has been learned or may be inferred respecting the Antarctic regions, to suggest the wish that further explorations may one day be undertaken. When we consider what has been done with sailing ships, it seems by no means unlikely that with steam-ships suitably constructed the Antarctic seas might be successfully explored. I would not encourage the idle ambition to penetrate so many miles farther southward than has hitherto been found practicable. But there are many and legitimate considerations in favour of further exploration. 'Within the periphery of the Antarctic circle,' says Captain Maury, 'is included an area equal in extent to one-sixth part of the entire land surface of our planet. Most of this immense area is as unknown to the inhabitants of the earth as the interior of one of Jupiter's satellites. With the appliances of steam to aid us, with the lights of science to guide us, it would be a reproach to the world to permit such a large portion of its surface any longer to remain unexplored. For the last 200 years, the Arctic Ocean has been a theatre for exploration; but as for the Antarctic, no expedition has attempted to make any persistent ex-

ploration, or even to winter there.* England, through Cook and Ross; Russia, through Billingshausen,

* I cannot refrain from touching here once again on the unfortunate circumstances relative to the transits of Venus in 1874 and 1882, because not only astronomy but geography must suffer seriously from them. When we consider what was about to be undertaken for the transit of 1882, and how small was the promise of astronomical results, even under the misapprehensions to which we have referred, we see how much might have been secured (even before this present time) if the more abundant promise of the earlier transit had been recognised in due time. In 1882 there are only two Antarctic stations to be thought of for a moment, and at one of these the sun will be only four degrees or so above the horizon at the moment when Venus enters on the sun's face, while at the other the sun will only be seven degrees above the horizon at that time. The least haze near the horizon, or the existence of mountains of moderate elevation lying on the south of the selected station (and it is suspected that lofty mountains exist in that direction), would render the observations futile. In 1874, on the contrary, there will be a high sun at three or four Antarctic stations, and every circumstance would tend to make the observations successful and useful. It has even been said, by one well qualified to express an opinion—to wit, by Commander Davis, who accompanied Sir James Ross in his southern voyages, and had himself landed at one of the stations suggested—that the meteorological chances of observing the transit would be *greatly more favourable* in this Antarctic station than at Kerguelen's Land. He considers, also, that there would be no difficulty whatever in again effecting a landing at the same place, viz. on Possession Island, off the coast of South Victoria, in latitude seventy-two degrees south; and that, with good huts, a party 'could pass the winter very comfortably, and would have a pleasant prospect before them and plenty of penguins to live on.' But to have secured the forwarding of such an expedition the attention of Government should have been directed to the matter as long before the transit of 1874 as in the actual case the transit of 1882 was anticipated (that is, in 1865, or thereabouts). Unfortunately, however, even at that very time, the mistake we have referred to led to the reiterated assertion that the transit of 1882 was alone worth observing at Antarctic stations; and again in 1868 the statement was repeated, that the method for which Antarctic voyages would alone be made 'fails totally for the transit of 1874' (*Monthly Notices of the Royal Astronomical Society*, vol. xxix. p. 33). It is now admitted that this was an over-hasty inference, but the admission comes too late. To

France, through D'Urville; and the United States, through Wilkes, have sent expeditions to the South Sea. They sighted and sailed along the icy barrier, but none of them spent the winter, or essayed to travel across and look beyond the first impediment. The expeditions which have been sent to explore unknown seas, have contributed largely to the stock of human knowledge, and they have added renown to nations, lustre to diadems. Navies are not all for war. Peace has its conquests, science its glories; and no navy can boast of brighter honours than those which have been gathered in the fields of geographical exploration or physical research.'

It does not appear that Antarctic voyages would be attended with any excessive degree of danger. No ship has hitherto been lost, we believe, in explorations beyond the Antarctic circle. It may be said, indeed, that such attempts are rather arduous than dangerous. It may even be found that the Antarctic barriers are impenetrable; but this has certainly not as yet been demonstrated. And it is far from being improbable that, if success could be achieved, an important field

observe the Transit of December 8, 1874, *successfully in the Antarctic regions, Possession Island should be occupied in January* 1874 *at the latest, by a party provided with the means of wintering there* (*the winter months being May, June, July, and August*). Unless our Australian cousins make the attempt, there is now, unfortunately, little hope of this being done. Government, at least, could scarcely be moved in time; though even now immortal honours might be gained by any who, having adequate means, should fit out a stout steam-ship for the purpose. The instrumental means, and astronomers to use them, would be forthcoming at once.

of commercial enterprise would be opened. The Antarctic regions are not mere desert wastes. The seamen under Ross found Possession Island covered by penguins standing in ranks like soldiers, and too little familiar with the ways of man to attempt escape. More valuable animals live and thrive, however, in Antarctic seas. Whales and seals exist there in abundance; and, as Captain Maury has well remarked, 'of all the industrial pursuits of the sea, the whale fishery is the most valuable.' In Arctic fisheries, he tells us, three thousand American vessels are engaged, and, 'if to these we add the Dutch, French, and English, we shall have a grand total of perhaps not less than six or eight thousand, of all sizes and flags, engaged in this one pursuit.' There are reasons for believing that whale fisheries in Antarctic regions would afford a richer, as they would certainly afford a far wider, field for maritime enterprise.

(From the *Cornhill Magazine* for March 1873.)

A FEW WORDS ABOUT COAL.

The recent rise in the price of coals has attracted a degree of attention to this useful mineral which it had not received so long as the scientific interest of the subject was alone in question. Geologists might discuss the rival theories which have been entertained respecting the origin of coal; or they might endeavour to

ascertain the probable distribution of the mineral, either as respects the extent of the earth's surface occupied by coal strata, or the geological depths at which it may be found; or lastly, statisticians might inquire into the probable duration of the supply of coal in particular regions; but the general public has hitherto paid no very close attention to these researches. Now, however, it is different; more particularly as respects the last of these questions. It has become a matter of serious import to many, to learn whether we may indeed look confidently for abundant supplies of coal during many future years, or whether those have been in the right who have told us that before the close of the present century this country must feel the effects of the over-rapid working of our chief coal-fields.

I propose briefly to sketch what is known about the origin of coal, and then to touch on the subject of the supply of this mineral, with special reference to the requirements of our own country.

A mistaken impression is somewhat widely prevalent that, in the coal-fields, we have the remains of ancient forests—in other words, it is supposed that wherever there was a forest in primeval times, there now exists a coal-field of greater or less extent. In connection with this view, also, the opinion is entertained that the forests now in existence will, in process of time, and after due geological changes, become the coal-beds of future ages.

But although, as we shall presently see, the coal-

fields are undoubtedly due to the vegetation of former eras, it is far from being the case that the primeval forests became converted in a general way into coal. Conditions of a peculiar, and to some extent exceptional, character were requisite for the formation of coal-fields. If we consider the evidence given by the coal-fields themselves, we shall see what these conditions were.

The beds or seams of coal form but a small portion of the thickness of the great geological group of strata to which they for the most part appertain. This group is called the *carboniferous*, and not uncommonly 'The Coal;' but even where coal is most abundant, it forms only a minute part of the whole mass. Thus it has been estimated, Sir Charles Lyell tells us, that in South Wales the thickness of the carboniferous strata amounts in all to between 11,000 and 12,000 feet (or more than two miles);* 'but the various coal-seams do not,' according to Professor Phillips, 'exceed in the aggregate 120 feet,' or little more than one-hundredth part of the whole. In North Lancashire the carboniferous strata occupy a depth of more than three and a half miles, with the same relative disproportion between the thickness of the coal-seams and that of the complete series of strata. Again, in Nova Scotia the coal-bearing strata attain a thickness of more than three

* It is, perhaps, hardly necessary to remark that this depth has not been measured anywhere in a vertical direction. The thickness of the several layers can be measured where they either crop out, or show at the surface, or else come within the range of mining operations; and thus the total depth of the series can be estimated.

miles.* Here no fewer than eighty seams of coal have been counted (seventy-one having been exposed by the action of the sea); but these seams are nowhere more than five feet in thickness, and many are but a few inches thick. Thus it is evident that the formation of coal can have been in progress but for a short portion of the time during which the great carboniferous series of strata was in process of deposition. Throughout by far the greater portion of that time other minerals were being deposited.

It is next to be noticed that under each coal-seam a stratum of ancient soil exists, in which there are commonly found the roots of ancient trees; while above the coal there is commonly a layer of shale or sandstone, in which not unfrequently the trunks of those trees are found either fallen or still in their original position, and only partly converted into coal. The bark remains, but is transmuted into coal; the hollow of the trunk, decaying long before the trunk gave way, is represented by a *cast* in sandstone. Thus, if we try to picture to ourselves the state of things which existed when such a seam of coal first began to be covered up by the next higher deposit, we see that there must have been trees standing erect above a layer

* The way in which this has been made known is worthy of notice. In the Bay of Fundy the tides run to an enormous height. The tidal wave can be seen when it is still thirty miles away, advancing with a prodigious uproar, and rising sometimes to the height of more than a hundred feet. These tremendous waves have not only produced a continuous section ten miles long, through the inclined strata, but by their action they sweep away continually the whole face of the cliffs, and bring into view fresh sections year after year.

of vegetable matter, the roots of the trees being imbedded in the soil which forms the deposit next below the coal. The vegetable layers may probably have been two or three times as thick as the resulting coal-seam, and were reduced by pressure to their present thickness; but such layers cannot at any time have reached to the branches of the forest-trees. Then the process of deposition began. This can only have happened when some subsidence of the soil had caused it to be submerged to a greater or less depth. We can infer from the depth of the strata overlying the coal-seams that this state of submergence continued in many cases for a long period of time; and it is equally clear that the formation of the vegetable layers themselves must have been a process occupying a considerable time, since tall trees grew before the next submergence took place. So soon as submergence was complete, the tall trees perished and began to decay. The stout trunks above the vegetable layer were broken off and swept away by the sea. The forest itself, properly so called, was for the most part thus destroyed. It was the decaying refuse of the forest, intermixed with the lowlier growths, which formed the coal-seam as it now exists. Amongst these were the lower parts of the trunks of the ancient forest-trees. These became converted, like the rest of the vegetable matter, into coal.

But it may be asked how those portions of the trunks which still remain above the level of the vegetable layer are to be accounted for. Are we to suppose that

they remained erect after the sea had made its way into the domain of the ancient forest? Many geologists think so; and doubtless the stumps of stout trees might resist for a long time the action of the sea waves. But there seems good reason for believing that, when the submergence first took place, these stumps stood but little above the upper surface of the vegetable layer, or that in many instances the trees were broken off even below that level. Then, as the pressure of the superincumbent layer gradually increased with the layer's increase of thickness, the vegetable matter was pressed down below its former level, and the stumps were left standing above the depressed surface of the vegetable layer. This explains the conversion of the bark of these stumps into coal, since there is every reason to believe that stumps simply left imbedded in sandstone would not change into pure coal.

In passing, I may remark that in whatever way it happened that the stumps of the ancient forest-trees remained standing above the level of the vegetable mass forming the coal-seam, a strange result has followed. The upper part of the stem became filled, as I have said, with sandstone, forming a cast of the interior of the ancient tree; the bark became coal; and outside the bark is sandstone again. Thus there is a mass of sandstone separated from the surrounding sandstone by a tube of coal. This mass is not cylindrical, being larger below than above; so that if in any way the mass ceases to be supported, it falls like a bolt from a gun. But in working the coal-seam the

material which had supported the sandstone mass is necessarily removed. Hence the miners look with dread on these coal-pipes, as they are called, which each year cause fatal accidents in the Newcastle and other coal-fields. As Sir Charles Lyell well remarks, 'it is strange to reflect how many thousands of these trees fell originally in their native forests in obedience to the law of gravity, and how the few which continued to stand erect, obeying, after myriads of ages, the same force, are cast down to immolate their human victims.'

We see, then, that coal-seams are the remains of ancient vegetable layers, formed underneath the trees of the ancient forest. But it is not to be supposed that every forest in those old times spread its shade over a mass of decaying vegetable matter, until the time should come when the mass should be covered over with shale or sandstone. In order that coal-seams should be formed, it was necessary that the forest region should be so abundantly watered as to form a forest swamp, like the cypress-swamps of the Mississippi. Yet again, it was necessary that during the fresh-water inundations which helped to accumulate the vegetable matter round the roots of the ancient forest-trees, no mud should be carried into the swamps. As Lyell says, 'one generation after another of tall trees grew with their roots in mud, and their leaves and prostrate trunks formed layers of vegetable matter, which was afterwards covered with mud, since turned to shale. Yet the coal itself, or altered vegetable matter, remained all the while unsoiled by earthy particles.

This is a fact which seems at a first view altogether perplexing; but, as nearly always happens with the more perplexing features of any natural enigma, geologists have been led by this difficulty to the interpretation of the enigma. It is to this very fact that we owe the most trustworthy information yet obtained respecting the process by which coal-beds were originally formed. The solution of the difficulty is due to the same eminent geologist from whom I have already quoted the statement of the difficulty. 'The enigma,' he says, 'however perplexing at first sight, may, I think, be solved by attending to what is now taking place in deltas. The dense growth of reeds and herbage which encompasses the margins of forest-covered swamps in the valley and delta of the Mississippi, is such that the fluviatile waters, in passing through them, are filtered and made to clear themselves entirely before they reach the areas in which vegetable matter may accumulate for centuries, forming coal if the climate be favourable. There is no possibility of the least intermixture of earthy matter in such cases. Thus, in the large submerged tract called the 'Sunk Country,' near New Madrid, forming part of the western side of the valley of the Mississippi, erect trees have been standing ever since the year 1811–1812, killed by the great earthquake of that date; lacustrine and swamp-plants have been growing there in the shallows, and several rivers have annually inundated the whole space, and yet have been unable to carry in any sediment within the outer boundaries of the morass, so

dense is the marginal belt of reeds and brushwood. It may be affirmed that generally in the 'cypress-swamps' of the Mississippi no sediment mingles with the vegetable matter accumulated there from the decay of trees and semi-aquatic plants. As a singular proof of this fact, I may mention that whenever any part of a swamp in Louisiana is dried up, during an unusually hot season, and the wood is set on fire, pits are burned into the ground many feet deep, or as far down as the fire can descend without meeting with water; and it is then found that scarcely any residuum or earthy matter is left. At the bottom of all these 'cypress-swamps' a bed of clay is found, with roots of the tall cypress, just as the under-clays of the coal are filled with *stigmaria*—the roots of the ancient forest-trees called *sigillaria*.*

It will be seen that the circumstances here considered dispose of the theory—once a favourite one

* It is not quite certain to what type of vegetation these trees belonged. They were formerly supposed to be tree-ferns, but some are found to have had long straight leaves, unlike those of ferns. The reader will probably remember that, after describing the *sigillaria*, the author of *Vestiges of Creation* describes the *stigmaria* as a distinct plant. 'Among the most remarkable of the leading plants of the coal era, without representatives on the present surface, are the *sigillaria*, of which large stems are very abundant, showing that the interior has been soft, and the exterior fluted, with separate leaves inserted in vertical rows along the flutings; and the *stigmaria*, a plant apparently calculated to flourish in marshes or pools, having a short, thick, fleshy stem, with a dome-shaped top, from which spring branches of from twenty to thirty feet long.' These branches were, in reality, the roots of the *sigillaria*. The mistake is a very natural one, since the coal-seam actually separates the trunk of the tree from its roots. Some, however, have since been found attached to the base of the tree-stumps.

with many geologists—that the coal-seams were formed of vegetable matter (the rubbish of decayed forests) which had been carried by rivers into estuaries and there formed into vast natural rafts. It was supposed that such rafts, sinking to the bottom, became after a while covered with a layer of sand or mud. The uprightness of the tree-stumps, however, as compared with the position of the coal-beds—that is to say, their position square to these beds—should of itself have disposed of the theory referred to.

Yet, on the other hand, there is great difficulty in understanding under what circumstances the alternate rising and sinking of the level of these delta-swamps, or morasses, took place during the enormously long period of time which must have been occupied in the formation of the carboniferous groups with a thickness amounting in some places to nearly four miles. We see, for instance, that in the case of the Nova Scotia coal-fields, there must have been eighty-one distinct submergences. Now there is nothing remarkable in the mere circumstance that the same part of the earth should have been above and beneath the sea-level through many successive alternations. Geology has long taught that in nearly every part of the earth this must have happened. But that throughout so many as eighty-one such changes those conditions should have been repeated which are necessary for the formation of coal-beds, is indeed a most remarkable circumstance. We have, on the one hand, the indications of a surprising degree of subterranean activity—for,

whether the land sank or the sea rose, there must have been a great oscillation of the earth's crust. But, on the other hand, we see that the great swamps must have retained their horizontal position unaltered for long periods of time. The growth of a forest is not the work of a few years, nor could the accumulations of vegetable matter have been formed quickly. As Lyell says, we have 'evidence of the former existence at more than eighty different levels'—overlying levels, be it noticed—'of forests of trees, some of them of vast extent, and which lasted for ages, giving rise to a great accumulation of vegetable matter. Under what condition must the earth's crust have been when such processes were possible? To this question, as yet, geology has given no satisfactory answer. There are considerations, however, which seem at least suggestive of a solution of some of the difficulties here presented.

It is, in the first place, a remarkable circumstance that, although vegetation was certainly not limited to the carboniferous period, yet it was in that period that all the chief coal-fields were formed. There are exceptions, no doubt, to this rule. In the times which preceded the carboniferous period, some coal-seams were formed, and some well-known coal-fields belong to later geological periods. There are beds of true coal belonging to the tertiary period (the latest of the main geological periods); and, passing from the oldest tertiary period to our own time, we find instances of the deposition of enormous quantities of *lignite* and *brown coal*,

as well as of the formation of *peat*, which must be regarded as only needing submersion and consequent pressure to become, in the lapse of time, either true coal or very near akin to it.* Yet, it remains true that the carboniferous group is the coal group *par excellence*; and when to this consideration is added the enormous thickness of the series of strata included in this group, we seem justified in concluding that this long period was characterised by some remarkable and distinctive peculiarity.

Now, whether we consider the lower portion of the carboniferous series, remarkable for the masses of limestone derived chiefly from animal substances, or the upper, where the coal-seams or vegetable layers abound, we find evidences of the presence of enormous quantities of carbon. In the upper part, the mere existence of a most abundant vegetation implies the presence of vast quantities of carbonic acid gas in the air. It seems not unlikely that this gas escaped from subterranean regions through the outlets formed by volcanoes; and the idea is suggested that the carboniferous period was one of great volcanic energy. In the older periods, there was probably a greater degree of subterranean activity, and from the carboniferous

* 'Near the surface,' says Professor Ansted, 'this substance (peat) is light-coloured and spongy, and the vegetable matter is little altered; deeper, it is brown, dense, and decomposed; at the bottom, it is black and nearly as dense as coal.' As a fuel, however, peat contains much ash. The same is true of lignite and brown coal. Moreover, brown coal is injured by exposure to the weather, which is not the case with true coal. Lignite splits in the air; brown coal falls to powder.

period onwards, even until our own, movements of the earth's crust have been probably more irregular and violent. But it would seem likely that, in the carboniferous period, an intermediate state of things prevailed when, owing to the greater heat of the earth's crust, and consequently the greater relative thickness of the plastic subterranean portions of the crust, the movements were more steady, and affected wider regions than at present, while the relief given by volcanic craters, instead of being intermittent as at present, was afforded uniformly and on a grander scale.

If this were, indeed, the case, then, towards the close of the carboniferous period, great disturbances of the earth's crust might be expected to have taken place, since that would be the time when the chief volcanic vents ceased to relieve the pent-up subterranean forces. This accords well with the condition of the geological record. 'The termination of the carboniferous formation,' says the author of the *Vestiges of Creation*, 'is marked by symptoms of volcanic violence' (by which he evidently means simply subterranean violence), 'which some geologists have considered to denote the close of one system of things and the beginning of another. Coal-beds generally lie in basins, as if following the curve of the bottom of the seas; but there is no such basin which is not broken up into pieces, some of which have been tossed up on edge, others allowed to sink, causing the ends of strata to be, in some instances, many yards, and, in a few, several hundred feet, removed from the corresponding ends of

neighbouring fragments. These are held to be results of volcanic movements below, the operation of which is further seen in numerous upbursts and intrusions of fire-born rock (trap). That these disturbances took place about the close of the formation, and not later, is shown by the fact of the next higher group of strata being comparatively undisturbed. Other symptoms of this time of violence are seen in the beds of conglomerate which occur among the first strata above the coal. These, as usual, consist of fragments of the elder rocks, more or less worn from being tumbled about in agitated water, and laid down in a mud paste, afterwards hardened.* It is to be admitted for strict truth' (rather a desirable object, by the way, in all such inquiries) 'that, in some parts of Europe, the carboniferous formation is followed by superior deposits, without the appearance of such disturbances between their respective periods; but apparently this case is exceptive. That disturbance was general is supported by the further and important fact of the destruction of many forms of organic being previously flourishing, particularly of the vegetable kingdom.'

It may be remarked in passing that the coal-seams are strikingly deficient in the fossil remains of animals.

* 'Volcanic disturbances,' adds our author in a note, 'break up the rocks; the pieces are worn in seams, and a deposit of conglomerate is the consequence. Of porphyry, there are some such pieces in the conglomerate of Devonshire, three or four tons in weight.' It is evident from this note, following, as it does, on the above passage as to the older rocks, that the writer is speaking of subterranean disturbances, not volcanic action, properly so-called; for volcanic action does not break up the older rocks; that is the work of earthquakes.

It is natural to ask, says Sir Charles Lyell, whether there were not air-breathing inhabitants of those forest regions where the accumulations of vegetable matter produced the coal-beds; but, if abundance of carbonic acid gas in the air were a main condition of the great vegetable wealth of the carboniferous period, the probability would seem to be that air-breathing creatures would be few, and those few of the lower orders of animal life. Certain it is that the poverty of the coal-seams in remains of animals has long been commented upon by geologists. We find footprints of a monstrous newt, or rather of an animal resembling the tadpole of the newt.* These creatures were truly amphibious, however, sharing the dominion of the water with the ganoid fishes—an association which 'reminds us,' says Lyell, 'that the living' creatures of the same order 'in America frequent the same rivers as the ganoids, the bony pikes.' They were undoubtedly powerful swimmers, Professor Huxley considers; and indeed, the main evidence we have of their having been air-breathers is the circumstance that *they left footprints on the sand.* If they had been walking under water, their weight would have

* The reader will be reminded of the suggestive remarks, by the author of the *Vestiges of Creation*, on similar tracks left by the *labyrinthodont* of Owen: 'That massive batrachian which leaves its hand-like footsteps in the new red sandstone, and then is seen no more. Not for nothing is it that we start at the picture of that strange impression—ghost of anticipated humanity—for apparently it really is so.' It need hardly be said, however, that this is not the view at present entertained by naturalists.

been so much reduced that they would have left no impressions, or only faint ones, whereas these are deep and distinct. They are not unlike the impressions which would be left by a small and rather plump hand. It is by no means clear that this creature ever made its way into the ancient forests, or could be in any proper sense regarded as their inhabitant.*

I have mentioned impressions left in sand belonging to the carboniferous period, and the ingenious way in which geologists have explained the features of these impressions. There is, however, a record on the sandstone of this period, which is, perhaps, even more significant. Impressions of rain-drops have been detected in carboniferous sandstone by Dr. Dawson, Sir Charles Lyell, and, more recently, by Mr. Brown, in Australia; and these rain-marks are, on the average, about as large as those which are produced by the rain of our own period. As Lyell well remarks, 'the great humidity of the climate of the coal period had been previously inferred from the number of its ferns, and the continuity of its forests for hundreds of miles; but

* Owing to the circumstance that in our books on geology this creature is called a batrachian, many popular writers have been led to assert that a monstrous frog inhabited the ancient forests whence the vegetation of the coal seams was derived. But the order of batrachians includes other animals than the frog and toad. According to the views at present adopted of the batrachian of the carboniferous period (as well as of a kindred but later species called by Professor Owen the *labyrinthodont*), this creature was further removed even than the newt from the common frog. It probably resembled in structure creatures still existing (but on a much smaller scale), which have four limbs like the newt, but have gills as well as lungs.

it is satisfactory to have at length obtained such positive proofs of showers of rain, the drops of which resembled in their average size those which now fall from the clouds. From such data, we may presume that the atmosphere of the carboniferous period corresponded in density with that now investing the globe, and that different currents of air varied then as now in temperature, so as to give rise, by their mixture, to the condensation of aqueous vapour.'

If we now turn to the consideration of the extent of the earth's surface occupied by those particular strata which belong to the coal period, we find evidence of the existence of enormous quantities of available coal. Professor Ansted mentions that a quarter of a million of square miles of the earth's surface 'are covered with sandstones and shales of the carboniferous period among which coal is buried; and this coal is for the most part accessible.' Now there are upwards of three million square yards of surface in a square mile; and, assuming an average total thickness of ten yards for all the distinct seams of each coal-field, we find for the total number of cubic yards of available coal the enormous figure 7,500,000,000,000. As a cubic yard of coal weighs nearly a ton, we may say that there are in round numbers seven billions of tons of coal available for the use of the human race. If we took the average number of human beings living at each moment during the next 3,500 years to be 2,000 millions, and the annual consumption for all purposes to be at the average rate of

one ton per human being, the supply would last for that enormous period.

But let us consider what portion of this vast supply falls to the share of this country—not including, of course, those coal-fields which lie in countries forming British territory, but not forming part of the British Isles; and let us compare our store of coals with our present rate of consumption and with the probable rate of consumption during coming years.

Some difficulty arises at the outset in determining what portion of the coal-fields actually existing in the British Isles may be regarded as available. We might, indeed, render the question more complicated by setting as a necessary part of the inquiry the determination of the actual expense per ton for mining, carriage, and so on, according as different parts of the coal-fields were being worked. But for obvious reasons this would not be the place for dealing with the subject in so general or so complete a manner. The sole point I shall here touch on, as bearing on the availability of the various coal-stores, is the probable depth to which coal-mining operations can be pushed.

It was held by many, ten years ago, that the coal-mines might be worked to a depth far exceeding the greatest which had then been reached. 'The difficulties in the way of deep mining,' wrote Mr. Leonard Lemoran, Surveyor of Mines, 'are mere questions of cost. It is important to notice that the assumption of 4,000 feet as the greatest depth to which coal can be worked, on account of the increase of tem-

perature, is purely voluntary. The increase has been calculated at a rate for which there is no authority; and while we are saying our coal-beds cannot be worked below 4,000 feet, a colliery in Belgium has nearly approached that depth, and no inconvenience is experienced by the miners.' But unfortunately this sanguine view has not been supported by recent researches. It will be known to our readers that in 1865 commissioners were appointed for discussing the whole subject of our coal supply. Among their ranks were several of the most eminent geologists, as well as some of the highest authorities on the practical questions involved in the subject. The question of the possible depth to which our mines could be worked, was necessarily one to which the commissioners were bound to give very close attention; and we may fairly accept the result of their inquiries as representing the most trustworthy conclusion which has yet been reached on this particular point. Now they stated that, according to the ordinary method of working, the depth at which the temperature of the mine would reach blood heat (or ninety-eight degrees Fahrenheit) is about 3,000 feet. They expressed a belief that, by the 'long-wall' system of working, a depth of 3,420 feet might be reached before this temperature was attained; but whether this will prove to be the case or not remains to be seen. Now, although the human frame can bear for a while a greater heat than 100 or even 200 degrees Fahrenheit,*

* Brewster mentions that Chantrey's workmen used to enter the furnace which the sculptor employed in drying his moulds, when the temperature was as high as 340°, 'walking over the floor with wooden

yet it would be impossible to carry on such labours as are required in coal-mining, at a higher temperature than blood heat, without great suffering and the loss of many lives.

Accordingly, although before the commissioners began their labours, the total quantity of available coal in Great Britain was reckoned at 200,000 millions of tons, it is now generally admitted that, so far as known coal-fields are concerned, the quantity probably available must be reckoned at something less than 150,000 millions of tons. The commissioners themselves found that in 1871 we had 'an aggregate of 146,480 millions of tons, which may be reasonably expected to be available for use.'

It will be observed that this quantity is about a forty-eighth part of the quantity probably available throughout the whole world; so that Great Britain possesses for her area a singularly large supply of the mineral.

Yet the consumption of coals in this country is so enormous that, though we are thus exceptionally well supplied, statisticians have already begun to look with anxiety upon the rapid exhaustion of our stores.

The questions at issue are exceedingly simple. Let it be granted that our total available supply amounts to 150,000 millions of tons. Then at our present rate of consumption, amounting nearly to 120,000,000 tons

clogs, which are, of course, charred on the surface.' Chantrey himself and five or six friends stayed for two minutes in the furnace, bringing out a thermometer which stood at 320°.

per annum, this supply would last the nation 1,250 years. But large as our consumption is, it is not the actual rate which is alarming, but the annual increase of rate. Year by year our consumption is increasing. Ten years ago it was under 84,000,000 tons, so that the average rate of increase during the ten years has been more than 3,500,000 tons. Taking it at only 3,000,000, the supply would not last 280 years. This is easily shown. For the increase at the supposed rate would, in 280 years, be no less than 840,000,000 tons, making a total annual consumption of 960,000,000. The mean between this and the present rate amounts to 540,000,000; and it will be found that 280 times 540 is greater than 150,000.

But startling as is the theory that our coal supply will be completely exhausted in less than 280 years—a period corresponding to that between the commencements of the reigns of Elizabeth and our present Queen—there are those who entertain an even more disheartening view. According to them it is not even the rate of increase of the annual consumption which forms the most threatening feature of the case, but the rate at which this rate of increase is itself increasing.

Thus Mr. Hull, dealing with the coal supply as we have just done, took 1,500,000 tons for the average annual increase (admittedly, however, a low estimate). We have seen that the present average annual increase cannot be set at less than 3,500,000. How if twenty years hence the average annual increase should have

risen to 5,000,000? half a century hence to 10,000,000? and so on. It is clear, at least, that if changes such as these take place in the rate of increase, we have greatly over-estimated in the above calculations the probable duration of our coal supply.

Mr. Stanley Jevons, in discussing the subject nine years ago, took the increase of increase into account with very startling results. He said: 'We, of course, regard not the average annual arithmetical increase of coal consumption, but the average rate per cent. of increase, which is found by computation to be 3·26.' Now, to illustrate the difference between this method and the other, we shall not take the actual figures, which are inconvenient for ready computation. Instead of doing so, we shall compare two simple progressions. One is the series 100, 110, 120, 130, 140, 150, and so on, increasing by ten at each step; the other is a progression increasing at the rate of ten per cent., and runs thus:—100, 110, 121, 133 (not counting fractions), 146, 161, and so on. It will be observed that the corresponding terms of the two series differ more and more from each other as we proceed: the difference is but one at the third term, and amounts to eleven at the sixth. It would be found to increase marvellously with a few more steps. Now, the difference between Mr. Stanley Jevons's method and Mr. Hull's is precisely analogous; only that whereas the rate per cent. just considered is *ten*, it is in the actual case about three and a quarter.

The fact really is, that the rate of increase corre-

sponds precisely, in one case, to that of a capital of 120,000,000 sterling, increased each year by simple interest at the rate of 3¼ per cent., while, in Mr. Jevons's method of calculation, the increase is as that of a capital of 120,000,000 sterling, increased by compound interest at the rate of 3¼ per cent. per annum. To give an idea of the actual difference as respects the consumption at some distant epoch, let us take the year 1950. Then, according to the former method of viewing the matter, the consumption in that year will be 373⅓ millions of tons; according to the latter, the consumption would be no less than 1,446 millions!

Startling as this result may seem, the commissioners found that in the last year of the five during which their labours continued, the consumption corresponded much more closely with the anticipations of Mr. Jevons than with the theory of an arithmetical rate of increase. And they remarked thereon that, though 'every hypothesis must be speculative, it is certain that, if the present rate of increase in the consumption of coal be indefinitely continued, even in an approximate degree, the progress towards the exhaustion of our coal will be very rapid.'

This will readily be believed when we mention that, according to Mr. Jevons's method of calculation, adopting 3·26 as the rate per cent. of increase, 150,000 millions of tons as the total available supply of coal, and 120,000,000 as the present annual consumption, our coals would not last us quite 127 years.

I venture, however, to indicate reasons for believ-

ing that the rate of increase here contemplated cannot possibly continue during many years, and that even the assumption of an arithmetical rate of increase at the present mean rate over-estimates the annual consumption for any time far removed from the present.

In all such progressions, statistics indicate a *wavelike* alteration. Just as in the shape of a wave's front, we see a gentle slope, then a more rapid slope, and then, up to the wave's summit, a gradually diminishing slope, so, in statistical progressions, we recognise a gradual increase at first, then a more rapid increase, then a diminishing increase, until the absolute maximum is reached, after which comes a gradual decrease. But the rear of such a statistical wave may be altogether unlike the front—in other words, the rate and manner and variations of decrease may be quite unlike the rate and manner and variations of increase. It is so with the progress of epidemics, with changes of population in the complete history of a nation from its rise to its decadence, with the growth of a trade, with every known subject to which statistical research has been applied. There may be alternate wavelike rise and fall, there may be so slow a rate of increase or decrease that the crest or valley of the wave seems long in passing, and the decrease after increase, or *vice versâ*, may so far differ from the preceding phase as to be almost imperceptible; but in every case there is to be recognised, either once or more than once, the wave-form of rise or of fall.*

* Let any one try such an experiment as the following, and he will

Now, the consumption of coal is at present, and for very obvious reasons, passing through the more rapidly-ascending portion of its wave of increase. For many years after the first recognition of the value of coal as fuel the mineral crept slowly into use. With its employment, fresh uses for it were found, the very usefulness of the mineral suggesting new wants. Chief among the results which sprang directly from the use of coal as fuel, was the application of the steam-engine to a number of purposes which had before been either unthought of or unattainable for want of proper fuel. The spread of manufacture, of trade, of travel, and general intercommunication, followed in due course, at once directly and indirectly necessitating a continued increase in the quantity of coal employed throughout the kingdom. These causes are still in full operation; and it is to be expected that, while this is the case, there will not merely be a steady annual increase in the use of coal (for such an increase would follow from

readily understand what is here meant by wave-like progression, and obtain also very convincing evidence of the fact in question. Along a horizontal line let equal spaces be measured, and let a set of vertical lines be pencilled through the divisions on the horizontal line. Now, from the weekly records of health let the number of deaths due to any disease, or form of disease—as, for instance, diarrhœa, or the class of diseases included under the head zymotic—be noted from the commencement to the end of some period in which such diseases may have been particularly active, and let the number of deaths in successive weeks be represented on any convenient scale on the successive vertical lines, measuring upwards from the horizontal line. [For example, say that 50 deaths shall be represented by one inch, and other numbers proportionately.] Then through the summits of the lines thus drawn let a curve be swept. It will be found that this curve has the wave-figure spoken of above.

the mere expansion of the uses already discovered for the mineral), but an increase of that increase, on account chiefly of the progress of invention and discovery.

That this state of things will continue for several years to come may fairly be anticipated; that for many years to come the average rate of increase in the coal-consumption will be fully equal to that at present observed may also be expected; but that, before many years are passed, the rate of increase (then higher than now) will be beginning to diminish, thenceforward returning towards its present rate, and passing eventually below its present rate, is to be looked forward to as the natural order of events in the future. Let it be remembered that such a result would by no means imply a falling off in the commercial and manufacturing activity of the country. The extension of the employment of coal for known uses has, in several instances, already nearly approached a limit. In other cases, such extension, though still proceeding, is not proceeding at an increasing but at a decreasing rate. This must happen in turn with all the known applications of coal, the extension of its use perhaps attaining a rate corresponding nearly to that of the actual growth of our population.* Such a change would imply a continual

* The population is increasing at an increasing rate at present; but as this rate is much lower than that at which the consumption of coal is increasing, this consumption, in changing to the rate at which population is increasing, must diminish its rate of increase. Moreover, the increase of the rate at which the population of this country is increasing, grows less, decade by decade.

increase of national commercial prosperity, not (as at first view might seem to be the case) a gradual decadence. It is as though a merchant, whose gains, already large, had been increasing year by year, say by 1,000*l.*, should find them still increase year after year by 900*l.*, 800*l.*, 700*l.* (the change occupying many years), until, at length, the annual increase settled down to some constant or nearly constant sum, such as (say) 200*l.* The prosperity of such a merchant could hardly be regarded as failing; for his gains, large at first, would have grown larger and larger throughout, and in the final stage they would still be growing larger and larger from year to year. So it would be in the case of those uses of coal which are already known. Already large, they would grow larger and larger (on our supposition, which we believe to be in accordance with all experience); they would not throughout the change fail to increase; and, at the last, they would settle down to a nearly constant rate, *not* of consumption, but of increase of consumption.

So soon as such processes begin to operate freely (and, as we have said, they are already operating to some extent) they will reduce the rate at which the whole consumption is increasing. Operating against them would be the progress of invention, by which fresh uses for coal are continually springing up. Yet this cause would not act *solely* to increase the consumption; for many of the inventions which require directly or indirectly the employment of coal, operate to remove or to reduce some other cause also requiring the consumption

of coal. Nor is it at all unlikely that before long inventions will be so directed as to reduce, in a very marked manner, the consumption of coal in certain departments of trade and commerce.

Now, if this view of the future is just, we can no longer apply a percentage of increase after Mr. Jevons's method, except for so moderate a number of years that the monstrous annual consumption indicated for 1950 (for example) is no longer in question. For the next ten, twenty, or even thirty years, it is not vitally important whether we take Mr. Jevons's method or Mr. Hull's. There would, indeed, be a considerable difference in the annual consumption at the end of the ten, twenty, or thirty years; but still the main difference would be that a certain consumption would be reached so many years sooner in one case than in the other.* Thus, taking 120 millions of tons as the present annual consumption, and 3·26 as the rate of increase per cent. per annum, the annual consumption ten years hence will be 159 millions of tons if the actual increase remain constant, and 165 millions of tons if the percentage of increase remain constant; at the end of twenty years the numbers will be respectively 185 millions and 227½ millions; at the end of

* It will be very soon in our power to decide whether one or other hypothesis be correct; nor will it be long before it will be possible to decide whether the hypothesis advocated by myself is not sounder than either. I venture to predict that before the year 1890 the percentage of increase will be markedly below Mr. Jevons's estimate; and that before the year 1900 the actual increase will be below its present value.

thirty years they will be 218 millions and 314 millions. The difference in the last two cases is no doubt considerable; yet it is seen that the consumption twenty years hence on Mr. Jevons's hypothesis is the same as the consumption thirty-three years hence on the other; and it can readily be calculated that the consumption thirty years hence on Mr. Jevons's hypothesis is the same as the consumption fifty years hence on the other. The advance to that rate twenty years earlier or later is a matter of very little importance compared with the question whether Mr. Jevons's view will be justified during after years.

If, on the contrary, as I believe, the present increasing rate of increase will be changed long before even thirty years have passed into a decreasing rate of increase—if such a consumption as 250 millions of tons is not reached until long after the time when even the present rate of change, continued uniformly, would have brought it—we need not fear that the exhaustion of our coal-fields is so near at hand as either Mr. Jevons or Mr. Hull has supposed. And we may recognise this further cause of hope in such a view, that, whereas the prospect of the exhaustion of our coal within 150 or 200 or even 300 years would imply little less than the prospect of approaching national bankruptcy, the continuance of our supply for 800 or 1,000 years would suffice to put us on a secure and stable footing. During all these years the power and wealth of the nation would be increasing, so far as the cause in question is concerned (since our assumptions imply a continual

increase in the consumption of coal); inventions and discoveries would have multiplied on all sides; means might even have been devised for accomplishing, without coal, the greater part of the work which coal now does for us; and at the worst we should be in a position to obtain abundant supplies of coal from other countries. It is not, however, too much to say that, even if *these* hopes were not justified, 1,000 years of prosperity is a future which this nation might contemplate with satisfaction. Whatever our pride in our country—in her past history, her present condition, or her future prospects—we are to remember that it is not given to any nation to endure for ever. As the most powerful nations of antiquity passed into decadence, so one day must it happen with this country, though we, her children, may well believe that that day is far off, and that the might and prosperity of this nation will rather undergo a change of form than a complete destruction—not perishing, but being merged in the might and the prosperity of one or other of the nations which have sprung from ours.

(From the *Cornhill Magazine* for October 1872.)

NOTES ON FLYING AND FLYING-MACHINES.

It would be difficult to say how many centuries have elapsed since the first attempt was made to solve the problem whether man can fly. Ages before the 'philosopher's stone' was ever sought for, or before the problem of perpetual motion had attracted the attention of mechanists, men had attempted to wing their way through the too unresisting air, by means of more or less ingenious imitations of the pinions of birds or insects. It has even been suggested (see Hatton Turnor's *Astra Castra*), that King David referred to successful attempts of this sort, when he cried, 'O that I had wings like a dove, then would I flee away and be at rest.' But without insisting on this opinion,—which indeed may be regarded as not wholly beyond cavil,—we have abundant evidence that in the earliest ages, the same problem has been attacked, which the Aëronautical Society of Great Britain took in hand but a few years since, and which, still more recently, the beleaguered Parisians sought earnestly, but in vain, to solve.

By the invention of the balloon the problem of aërial *floatation* has been solved; but the problem which has hitherto proved so intractable, is that of aërial *navigation* or flight,—whether by means of flying-machines capable of supporting many persons at once, or by means of contrivances enabling a man to urge his way alone through the air. There can be

little question that this problem is one of great difficulty. It has, indeed, been long regarded by nearly all practical mechanicians as really insoluble. But of late years careful researches have led competent men to entertain doubts as to the validity of the objections which have been urged against the theory that it is possible for men to fly. Facts have come to light which seem, to say the least, highly promising. In fine, there are not a few who share the convictions of the learned president of the Aëronautical Society, that before many years have passed men will have learned how to navigate the air. The time may not be at hand, indeed, when Bishop Wilkins's prophecy will be fulfilled, and men will call as commonly for their wings, as they now do for their boots; but it does not seem improbable that before long the first aërial voyage (as distinguished from aërial drifting in balloons) will be successfully accomplished.

It may be interesting to inquire, what are the principal facts on which this hopeful view of the long-vexed problem has been founded. In so doing, I shall have occasion to touch incidentally on the history of past attempts at flight; and this history is, indeed, so attractive, that the reader may be disposed to wish that it were entered upon more at length. But my subject is such a wide one, that it will be necessary to avoid discussing, at any length, those strange, and sometimes apocryphal narratives, which are to be found in the records of aëronautics. For this reason I propose to consider only such accounts of past attempts, as appear

to bear on the subject of the actual feasibility of flying.

In the problem of aërial navigation, four chief points have to be considered—buoyancy, extent of supporting surface, propulsive power, and elevating power. At first sight, buoyancy may seem to include elevating power and supporting power, but it will be seen, as we proceed, that the term is used in a more restricted sense.

In the balloon we have the perfect solution of the problem of securing buoyancy. The success with which men have overcome the difficulty of rising into the air is complete; and this being their first, and seemingly, a most important success, we can perhaps hardly wonder that further success should long have been looked for in the same direction. The balloon had enabled men to float in the air; why should it not enable them also to direct their course through the air? The difficulty of rising into the air seemed, indeed, much the more serious of the two before the balloon had been invented; and all who had failed in their attempts to fly, had failed in precisely this point.

Yet all attempts to direct balloons have hitherto failed. It seems clear, indeed, when we inquire carefully into the circumstances of the case that such attempts must necessarily fail. The buoyancy of balloons is secured, and can be secured, only by one method, and that method is such as to preclude all possibility—so at least it seems to me—that the

balloon can be navigated. A balloon must be large,—many times larger than any machine to which it can be attached. If we take even the case of one man raised by a balloon, and inquire how large the balloon should be, we at once see how disproportioned the size of a balloon must needs be to the bodies of a heavier nature which it is intended to raise. We know that a man can barely float in water, so that he is about equal in weight to an equal volume of water. But a volume of water is more than eight hundred times heavier than an equal volume of air, even at the sea-level, where the air is densest. So that the weight of a man is more than eight hundred times greater than that of the air he displaces. It follows that if a very light hollow vessel could be made, which should be more than eight hundred times as large as a man, and which could be perfectly exhausted of air without collapsing (a thing wholly impossible), the buoyancy of that vessel would barely enable it to support the weight of a man. But the balloonist is unable to obtain any vessel of this sort. He cannot employ the buoyancy of a perfect vacuum to raise him. What he has to do, is to fill a silken bag with a gas lighter than air, but still not weightless, and to trust to the difference between the weight of this gas and that of the air the balloon displaces, to raise him from the ground. So that such a balloon, in order to raise a man, must be considerably larger than the hollow vessel just referred to. But further, the balloon must rise above the denser parts of the air; it must carry its own weight as well as that of

the man; the balloonist must take a supply of ballast; and other like considerations have to be attended to, all of which render it necessary that the balloon should be larger than we have hitherto supposed. Apart, however, from all such considerations, we find the very least proportion between the size of the balloon intended to carry one person, and the size of the human body, to be about as one thousand to one. Buoyant vessels constructed on such a scale must needs present an enormous surface; and therefore, not only must they strongly resist all attempts made to propel them in any direction, but the lightest wind must have more effect upon them than any efforts made by those they carry. As for any power which should avail to propel a balloon against a strong wind, the idea seems too chimerical to be entertained. Until men can see their way to propelling a buoyant body (one thousand times larger than the weight it supports), at the rate of fifteen or twenty miles an hour through calm air, they cannot expect even to resist the action of a steady breeze on a balloon, far less to travel against the wind. But even if it were possible to conceive of any contrivance by which a balloon could be propelled rapidly through calm air, yet the mere motion of the balloon, at such a rate, would sway the balloon from its proper position, and probably cause its destruction. A power which could propel the car of a balloon through calm air at the rate of twenty miles an hour, would cause precisely the same effect on the balloon itself, as though the car were fixed, while a heavy wind was blowing

against the balloon. We know what the effect would be in this latter case; the balloon would soon be made a complete wreck: and nothing else could happen in the former case.

But it may be seriously questioned, whether buoyancy is a desirable feature in any form of flying-machine. We have seen that a degree of buoyancy sufficient to secure actual floatation in the air is incompatible with aërial navigation. We may now go further, and urge that even a less degree of buoyancy would be a mischievous feature in a flying-machine. M. Nadar, the balloonist, makes a significant, though not strictly accurate observation on this point, in his little book on flying. Passing through the streets of Paris, during the ædileship of Haussmann, he heard a workman call from the roof of a house to a fellow-workman below, to throw a sponge up. 'Now,' says Nadar, 'what did the cunning workman, who was to throw the sponge up, do? The sponge was dry, and therefore light and buoyant. Was it in this condition that he threw it up to his fellow? No; for it would not have been possible to send it above the first floor. But he first wets the sponge; and so makes it heavy; and then, when it has been deprived of the lightness which is fatal to flight, he throws it easily to his fellow on the house-roof?' M. Nadar infers, that the first essential in a flying-machine is weight!

Now what is true in the above reasoning is that buoyancy renders flight—as distinguished from aërial floating—impossible, or at least difficult. It is not

true, however, that the flight of the wet sponge exemplifies the kind of flight which the aëronaut requires. The sponge, in fact, was neither more nor less than a projectile; and most assuredly, the problem of flight is not to be solved by making projectiles of our flying-machines or of our own bodies. It may be, and indeed we shall presently see that it probably *will* be necessary, that some form of propulsion from a fixed stand should have to be applied to the flying-machines of the future. But after such propulsion has been applied, the flying-machine must be *supported* in some way, not left—as an ordinary projectile is left—to the action of unresisted gravity. M. Nadar's wet sponge is no analogue, then, of the flying-machines we require.

Before leaving the subject of buoyancy, however, it will be desirable to inquire whether buoyancy is, in any marked degree, an attribute of the flying creatures we are acquainted with—birds, bats, and insects. The structure of such creatures has been supposed by some to be such as to secure actual buoyancy, to a greater or less degree; and many would be disposed, at a first view of the matter, to regard the hollow bones and the quill-feathers of birds as evidences that buoyancy is essential to flight. I have even seen the strange theory put forward, that during life, the quills of birds, as well as their hollow bones, are filled with hydrogen. 'Flying animals,' says a writer in *All the Year Round* for March 7, 1868, 'are built to hold gases everywhere—in their bones, their bodies, their skins; and

as their blood is several degrees warmer than the blood of walking or running animals, their gases are probably several degrees lighter. Azote, or hydrogen, or whatever the gas held in the gaseous structures may be, it is proportionately warmer, and therefore proportionately lighter than air.'

But it appears to me that on a careful consideration of the structure of flying creatures, the hollow portions of their bodies will be found to fulfil a purpose quite distinct from that of imparting buoyancy. If we examine a quill we find that the most remarkable feature which it presents to us, is the proportion which its strength, especially as respects resistance to flexure, bears to its weight. It would be difficult, indeed, to construct any bar, or rod, or tube, of the same length and weight as a portion of a bird's quill, which would bear the same pressure without perceptible flexure; and it is scarcely conceivable that any structure appertaining to a living creature, could possess greater strength with an equal degree of lightness. In the hollow bones, again, we see the same association of strength and lightness. Precisely as a tubular bridge, like that which spans the Menai Straits, is capable of bearing far greater strain than a solid metal bar of equal weight and length, so the hollow bones of birds are far stronger than solid bones of equal weight would be. We see then, that *lightness* is secured in these parts of a bird's structure. But lightness and buoyancy are different matters. We can understand that it is absolutely essential, that the

weight of a machine intended for flight should be as small as may be, due regard being had to strength and completeness. But there is little, I conceive, in the structure of flying creatures, which points to buoyancy as a desirable feature in a flying-machine.

We come next to a much more important point, namely, extent of supporting surface. We are to consider the air now, not with regard to its density, the quality which enables a balloon, filled with rarer gas, to float in air, but with reference to its power of resisting downward motion through it; that is, of resisting the effects of gravity. We have to inquire what extent of surface, spread either in the form of wings or as in parachutes, will suffice to support a man or a flying-machine. It is here that the researches recently made seem to bear most significantly upon the question of the possibility of flight.

The history of the parachute affords some insight into the supporting power of the air—some, but not much. The parachute has been commonly suffered to fall from beneath the car of a balloon. Suspended thus, in the lee, so to speak, of the balloon's mass, and with its supporting surface unexpanded, the parachute descends under highly unfavourable conditions. A great velocity of descent is acquired before the parachute is fully expanded, and thus the parachute has to resist a greater down-drawing force than would be the case if the machine were open, and surrounded on all sides by free air, at starting. The consequence is a great and sudden strain upon all parts of the

parachute, as well as a degree of oscillation which seriously risks its structure, besides impairing its supporting power—since this power would obviously act most effectively if the span of the parachute remained horizontal throughout the descent. The following account of Garnerin's descent, in 1797, illustrates the foregoing remarks:—'In 1797,' says Mr. Manley Hopkins, 'Garnerin constructed a parachute, by which he descended from a balloon, at an elevation of 2,000 feet. The descent was perilous, for the parachute failed, for a time, to expand; and after it had opened, and the immediate fears of the immense concourse which had assembled in Paris to witness the attempt, had been removed, the oscillations of the car, in which Garnerin was seated, were so violent as to threaten either to throw him out, or, on arriving at the ground, to dash him out with violence. He escaped, however!" We notice the same circumstances in the narrative of poor Cocking's disastrous attempt in 1837. 'When the cords which sustained the parachute were cut, it descended with dangerous rapidity, oscillating fearfully, and at last the car broke away from the parachute, and Mr. Cocking was precipitated to the ground, from a height of about one hundred feet.'

But apart from these considerations, the parachute affords no evidence whatever of the increased sustaining power of the air on bodies which traverse it rapidly in a more or less horizontal direction. The parachute descends, and descends quickly: we have to inquire

whether the air may not resist descent so strongly that with comparatively small effort a horizontal or even ascending motion may be effected.

A familiar illustration of this supporting power of the atmosphere is given in the flight of an oyster-shell or piece of thin slate, deftly thrown from a schoolboy's practised hand. Such a missile, instead of following the parabolic path traversed by an ordinary projectile, is seen to skim along almost like a bird on resting pinions. It will sometimes even ascend (after the projectile force has ceased to act in raising it), as though in utter disobedience to the laws of gravitation.

The fact appears to be, that when a horizontal plane traverses the air in a horizontal direction, the supporting power of the air is increased in proportion as the plane moves more quickly, or in proportion to the actual quantity of air it glides over, so to speak. Indeed, we have clear evidence to this effect in the behaviour of the common toy-kite, the supporting power of which is increased in proportion to the force of the wind. For a kite held by a string in a strong horizontal current of air, corresponds exactly to an inclined plane surface drawn swiftly in a horizontal direction during a calm. The same supporting power which results from the rapid passage of the air under the kite will be obtained during the rapid passage of the kite over still air.

When we study the flight of birds we are confirmed in the opinion that velocity of horizontal motion

is a point of extreme importance as respects the power of flying. For though there are some birds which seem to rise almost straight from the ground, yet nearly all, and especially the larger and heavier birds, have to acquire a considerable horizontal velocity before they can take long flights. Even many of those birds which seem, when taking flight, to trust rather to the upward and downward motion of their wings than to swift horizontal motion, will be found, when carefully observed, to move their wings up and down in such sort as to secure a rapid forward motion. I have been much struck by the singularly rapid forward motion which pigeons acquire by what appears like a simple beating of their wings. A pigeon which is about to fly from level ground may be seen to beat its wings quickly and with great power; and yet instead of rising with each downward stroke, the bird is seen to move quite horizontally,—as though the wings acted like screw-propellers. I believe, in fact, that the wings during this action do really act, both in the upward and downward motion, in a manner resembling either screw-propulsion or the action by which seamen urge a boat forward by means of a single oar over the stern.* The action of a fish's tail is not dissimilar; and as the fish, by what seems like a simple beating of its tail from side to side, is able to dart swiftly forwards, so the bird, by what seems like a beating of its wings up and down, is able—when occa-

* Sailors call this *sculling*, a term more commonly applied to the propulsion of a boat by a single oarsman using a pair of oars, or sculls.

sion requires—to acquire a swift forward motion. At the same time it must be understood that I am not questioning the undoubted fact that the downward beat of a bird's wing is also capable of giving an upward motion to the bird's body. The point to be specially noticed is that when a bird is taking flight from level ground, the wings are so used that the downward stroke gives no perceptible upward motion.

But since a horizontal velocity is thus effective, we might be led to infer that the larger flying creatures, which, *cœteris paribus*, travel more swiftly through the air than the smaller, would require a smaller relative extent of supporting surface. We are thus led to the consideration of that point which has always been regarded as the great, or rather the insuperable difficulty, in the way of man's attempts at flight,—his capacity or incapacity to carry the requisite extent of supporting surface. We are led to inquire whether a smaller extent of supporting surface than has hitherto been deemed necessary may not suffice in the case of a man, and *à fortiori* in the case of a large and powerful flying-machine.

The inference to which we have thus been led, is found to accord perfectly with the observations which have been made upon flying creatures of different dimensions. It has been found that the supporting surface of these creatures,—whether insects, birds, or bats,—by no means varies in proportion to their weight. This is one of the most important results to which the recent inquiries into the problem of flight have led; and I believe that my readers cannot fail to be inte-

rested by an account of the relations which have been observed to hold between the weight and the supporting surface of different winged creatures.

We owe to M. de Lucy, of Paris, the results of the first actual experiments carried out in this direction. The following account of his observations (made in the years 1868, 1869) is taken from a paper by Mr. Brearey, the Honorary Secretary to the Aëronautical Society. 'M. de Lucy asserts,' says Mr. Brearey, 'that there is an unchangeable law, to which he has never found any exception, amongst the considerable number of birds and insects whose weights and measurements he has taken,—viz., that the smaller and lighter the winged animal is, the greater is the comparative extent of supporting surface. Thus in comparing insects with one another—the gnat, which weighs 460 times less than the stag-beetle, has 14 times greater relative surface. The lady-bird, which weighs 150 times less than the stag-beetle, possesses 5 times more relative surface, &c. It is the same with birds. The sparrow, which weighs about 10 times less than the pigeon, has twice as much relative surface. The pigeon, which weighs about 8 times less than the stork, has twice as much relative surface. The sparrow, which weighs 339 times less than the Australian crane, possesses 7 times more relative surface, &c. If we now compare the insects and the birds, the gradation will become even more striking. The gnat, for example, which weighs 97,000 times less than the pigeon, has 40 times more relative surface; it weighs 3,000,000 times less than the crane

of Australia, and possesses relatively 140 times more surface than this latter, which is the heaviest bird M. de Lucy had weighed, and was that also which had the smallest amount of surface, the weight being nearly 21 lbs.; and the supporting surface 139 inches per kilogramme (2 lbs. 3¼ oz.). Yet of all travelling birds the Australian cranes undertake the longest and most remote journeys, and, with the exception of the eagles, elevate themselves the highest, and maintain flight the longest.'

M. de Lucy does not seem to have noticed the law to which these numbers point. It is exceedingly simple, and amounts in fact merely to this, that instead of the wing-surface of a flying creature being proportioned to the weight, it should be proportioned to the surface of the body (or technically, that instead of being proportioned to the cube, it should be proportioned to the square of the linear dimensions). Thus, suppose that of two flying creatures one is 7 times as tall as the other, the proportions of their bodies being similar, then the body surface of the larger will be 49 times (or 7 times 7) that of the other, and the weight 343 times (or 7 times 7 times 7) that of the other. But instead of the extent of wing-surface being 343 times as great, it is but 49 times as great. In other words, relatively to its weight the smaller will have a wing-surface 7 times greater than that of the larger. How closely this agrees with what is observed in nature, will be seen, by the case of the sparrow as compared with the Australian crane; for M. de Lucy's experiments

show that the sparrow weighs 339 times less than the Australian crane, but has a relative wing-surface 7 times greater.

It follows, in fact, from M. de Lucy's experiments that, as we see in nature, birds of similar shape should have wings similarly proportioned, and not wings corresponding to the relative weight of the birds. The same remark applies to insects; and we see, in fact, that the bee, the bluebottle, and the common fly—insects not unlike in their proportions—have wings proportioned to their surface dimensions; the same holding amongst long-bodied insects, like the gnat and the dragon-fly, and the same also among the different orders of flying beetles.

So that, setting apart differences of muscular capacity and adaptation, a man, in order to fly, would need wings bearing the same proportion to his body as we observe in the wings of the sparrow or the pigeon. In fact, the wings commonly assigned to angels by sculptors and painters would not be so disproportioned to the requirements of flight as has been commonly supposed, if only the muscular power of the human frame were well adapted to act upon wings so placed and shaped, and there were no actual inferiority in the power of human muscles (cross-section for cross-section) as compared with those of birds.

So far as the practicability of actual flight on man's part is concerned, these two points are, indeed, among the most important we have to consider. It was to Borelli's remarks on these points, in his famous treatise, *De*

Motu Animalium, that the opinion so long entertained respecting the impracticability of flight must be referred. He compared the relative dimensions of the breast-muscles of birds with those of the corresponding muscles in man, and thence argued that man's frame is altogether unadapted to the use of wings. He compared also the relative muscular energy of birds and men, that is, the power of muscles of equal size in the bird and the man; and was yet further confirmed in the opinion that man can never be a flying animal.

But although the reasoning of Borelli suffices perfectly well to show that man can never fly by attaching pinions to his arms, and flapping these in imitation (however close) of a bird's action in flying, it by no means follows that man must be unable to fly when the most powerful muscles of his body are called into action to move suitably-devised pinions. M. Besnier made a step in this direction (towards the close of the last century) when he employed, in his attempts to fly, those powerful muscles of the arm which are used in supporting a weight over the shoulder (as when a bricklayer carries a hod, or when a countryman carries a load of hay with a pitchfork). But the way in which he employed the muscles of the leg was less satisfactory. In his method, a long rod passed over each shoulder, folding pinions being attached to both ends of each rod. When either end of a rod was drawn down, the descending pinion opened, the ascending pinion at the other end closing; and the two rods were worked by alternate downward pulls with the arms and legs. The

downward pull with the arms was exceedingly effective; but the downward pull with the legs was altogether feeble. For the body lying horizontally, the muscles used in the downward pull with the legs were those by which the leg is carried forward in walking, and these muscles have very little strength, as any one will see who, standing upright on one leg, tries, without bending the knee of the other, to push forward any considerable weight with the front of this leg.

Yet even with this imperfect contrivance Besnier achieved a partial success. His pinions did not, indeed, serve to raise him in the air; but when, by a sharp run forward, he had brought that aërial supporting power into action of which we have spoken above, the pinions, sharply worked, so far sustained him as to allow him to cross a river of considerable width. It is not unlikely that, had Besnier provided fixed sustaining surfaces, in addition to the moveable pinions, he might have increased the distance he could traverse. But, as regards flight, there was a further and much more serious defect in his apparatus. No means whatever were provided for propulsion. The wings tended to raise the body (this tendency only availing, however, to sustain it); but they could give no forward motion. With a slight modification, it is probable that Besnier's method would enable an active man to travel over ground with extreme rapidity, clearing impediments of considerable height, and taking tolerably wide rivers almost 'in his stride;' but I believe that the method could never enable men actually to fly.

It may be remarked, indeed, that the art of flying, if it is ever attained, will probably be arrived at by means of attempts directed, in the first place, towards rapid passage along *terra firma*. As the trapeze gymnast avails himself of the supporting power of ropes, so the supporting power of the air may be called into action to aid men in traversing the ground. The following passage from Turnor's *Astra Castra* shows that our velocipedists might soon be outvied by half-flying pedestrians: 'Soon after Bacon's time,' he tells us, 'projects were instituted to train up children from their infancy in the exercise of flying with artificial wings, which seemed to be the favourite plan of the artists and philosophers of that day. If we credit the accounts of some of these experiments, it would seem that considerable progress was made that way. The individuals who used the wings could skim over the surface of the earth with a great deal of ease and celerity. This was accomplished by the combined faculties of running and flying. It is stated that, by an alternate continued motion of the wings against the air, and the feet against the ground, they were enabled to move along with a striding motion, and with incredible speed.

A gymnast of our own day, Mr. Charles Spencer ('one of the best teachers of gymnastics in this country,' says Mr. Brearey), has met with even more marked success, for he has been able to raise himself by the action of wings attached to his arms. The material of which these wings were made was too fragile for actual flight; and Mr. Spencer was prevented

from making strong efforts because the wicker-work to which the apparatus was attached, fitting tightly round his body, caused pain, and obstructed his movements. Yet he tells us that, running down a small incline in the open air, and jumping from the ground, he has been able, by the action of the wings, to sustain flight for a distance of 120 feet; and when the apparatus was suspended in the transept of the Crystal Palace (in the spring of 1868), he was able, as we have said, to raise himself, though only to a slight extent, by the action of the wings. It should be remarked, however, that his apparatus seems very little adapted for its purpose, since the wings are attached to the arms in such sort that the weak breast-muscles are chiefly called into play. Borelli's main objection applies in full to such a contrivance; and the wonder is that Mr. Spencer met with even a partial success. One would have expected rather that the prediction of a writer in the *Times* (calling himself Apteryx, or the Wingless) would have been fulfilled, and that the 'aëronaut, if he flapped at all, would come to grief, like the sage in "Rasselas," and all others who have tried flying with artificial wings.'

The objection founded on the relative weakness of the muscles of man as compared with those of birds (without reference to the question of adaptation), seems at first sight more serious. Although there can be little question that the superior strength of the muscles of birds has been in general enormously exaggerated, yet such a superiority undoubtedly exists to

some degree. This gives the bird a clear advantage over man, insomuch that man can never hope by his unaided exertions to rival the bird in its own element. It by no means follows, however, that because man may never be able to rival the flight of the eagle or the condor, of the pigeon or the swallow, he must therefore needs be unable to fly at all.

It should be remembered, also, that men can avail themselves of contrivances by which a considerable velocity may be acquired at starting; and that when the aëronaut is once launched with adequte velocity, a comparatively moderate exertion of force may probably enable him to maintain that velocity, or even to increase it. In this case, a moderate exertion of force would also suffice to enable him to rise to a higher level. To show that this is so, we need only return to the illustration drawn from the kite. If a weight be attached to a kite's tail, the kite, which will maintain a certain height when the wind is blowing with a certain degree of force, will rise to a greater height when the force of the wind is but slightly increased.

Kites afford, indeed, the most striking evidence of the elevating power resulting from the swift motion of an inclined plane through the air, the fact being remembered always that, whatever supporting and elevating power is obtained when air moves horizontally with a certain velocity against an inclined plane, precisely the same supporting and elevating power will be obtained when the inclined plane is drawn or propelled horizontally with equal velocity through still air.

Now the following passages from the 'History of the Char-volant,' or kite-carriage, bear significantly on the subject we are now upon. The kite employed in the first experiments (made early in the present century) had a surface of fifty-five square feet. 'Nor was less progress made in the experimental department when large weights were required to be raised or transposed. While on this subject, we must not omit to observe that the first person who soared aloft in the air by this invention was a lady, whose courage would not be denied this test of its strength. An arm-chair was brought on the ground, then, lowering the cordage of the kite by slackening the lower brace, the chair was firmly lashed to the main-line, and the lady took her seat. The main-brace being hauled taught, the huge buoyant sail rose aloft with its fair burden, continuing to ascend to the height of a hundred yards. On descending, she expressed herself much pleased with the easy motion of the kite and the delightful prospect she had enjoyed. Soon after this another experiment of a similar nature took place, when the inventor's son successfully carried out a design not less safe than bold—that of scaling by this powerful aërial machine the brow of a cliff two hundred feet in perpendicular height. Here, after safely landing, he again took his seat in a chair expressly prepared for the purpose, and, detaching the swivel-line which kept it at its elevation, glided gently down the cordage to the hand of the director. The buoyant sail employed on this occasion was thirty feet in height, and had a proportionate

spread of canvas. The rise of the machine was most majestic, and nothing could surpass the steadiness with which it was manœuvred, the certainty with which it answered the action of the braces, and the ease with which its power was lessened or increased. . . Subsequently to this, an experiment of a very bold and novel character was made upon an extensive down, where a waggon with a considerable load was drawn along, whilst this huge machine at the same time carried an observer aloft in the air, realising almost the romance of flying.'

We have here abundant evidence of the supporting and elevating power of the air. This power, is however, in a sense, dormant. It requires to be called into action by suitable contrivances. In the kite, advantage is taken of the motion of the air. In flight, advantage must be taken of motion athwart the air, this motion being, in the first place, communicated while the aëronaut or flying-machine is on the ground. Given a sufficient extent of supporting surface, and an adequate velocity, any body, however heavy, may be made to rise from the ground; and there can be no question that mechanicians can devise the means of obtaining at least a sufficient velocity of motion to raise either a man or a flying-machine, provided with no greater extent of supporting surface than would be manageable in either case. It is not the difficulty of obtaining from the air *at starting* the requisite supporting power that need deter the aëronaut. The real difficulties are those which follow. The velocity of motion must

be maintained, and should admit of being increased. There must be the means of increasing the elevation, however slowly. There must be the means of guiding the aëronaut's flight. And lastly, the aëronaut or the flying machine must fly with well-preserved balance—the supporting power of the air depending entirely on the steadiness with which the supporting surfaces traverse it.

I believe that these difficulties are not insuperable; and not only so, but that none of the failures recorded during the long history of aëronautical experiments need discourage us from hoping for eventual success. Nearly all those failures have resulted from the neglect of conditions which have now been shown to be essential to the solution of the problem. Nothing but failure could be looked for from the attempts hitherto made; and, indeed, the only wonder is that failure has not been always as disastrous as in the case of Cocking's ill-judged descent. If a man who has made no previous experiments will insist on jumping from the summit of a steeple, with untried wings attached to his arms, it cannot greatly be wondered at that he falls to the ground and breaks his limbs, as Allard and others have done. If, notwithstanding the well-known weakness of the human breast-muscles, the aëronaut tries to rise by flapping wings like a bird's, we cannot be surprised that he should fail in his purpose. Nor again can we wonder if attempts to direct balloons from the car should fail, when we know that the car could not even be drawn with ropes against a steady breeze without injury to the supporting

balloon. And we need look no further for the cause of the repeated failures of all the flying-machines yet constructed, than to the fact that no adequate provision has yet been made to balance such machines, so that they may travel steadily through the air. It seems to have been supposed that if propelling and elevating power were supplied, the flying-machine would balance itself; and accordingly, if we examine the proposed constructions, we find that in nine cases out of ten (if not in all) the machine would be as likely to travel bottom-upwards as on an even keel. The common parachute (which, however, is not a flying-machine) is the only instance I can think of in which a non-buoyant machine for aërial locomotion has possessed what is called a 'position of rest.'

Perhaps the gravest mistake of all is that of supposing that, on a first trial, a man could balance himself in the air by means of wings. Placed, for the first time, in deep water, man is utterly unable to swim, and if left to himself will inevitably drown; although a very slight and very easily acquired knowledge of the requisite motions will enable him to preserve his balance. And yet it seems to have been conceived by most of those who have attempted flight, that when first left to himself in open air, with a more or less ingeniously contrived apparatus attached to him, a man would not only be able to balance himself in that unstable medium, but also to resist the down-drawing action of gravity (which scarcely acts at all on the swimmer), and wing his way through the air by a series of new and untried movements!

It encourages confidence in the attempts now being made to solve the problem of aërial locomotion, that they are tentative, — founded on observation and experiment, and not on vague notions respecting the manner in which birds fly. Fresh experiments are to be made, more particularly on the supporting power of the air upon bodies of different form, moving with different degrees of velocity. These experiments are under the charge of Messrs. Browning and Wenham, of the Aëronautical Society, whose skill in experimental research, and more particularly in inquiries depending on mechanical considerations, will give a high value to their deductions. The question of securing the equipoise of flying-machines has also received attention; and it is probable that the principle of the instrument called the gyroscope will be called into action to secure steadiness of motion, at least in the experimental flights. What this principle is need not here be scientifically discussed. But it may be described as the tendency of a rotating body to preserve unchanged the direction of the axis about which the body is rotating. The spinning-top and the quoit (well thrown), afford illustrations of this principle. The peculiar flight of a flat missile, already referred to, depends on the same principle; for the flight only exhibits the peculiarities mentioned when the missile is caused to whirl in its own plane. But the most striking evidence yet given of the steadying property of rotation, is that afforded by the experiments of Professor Piazzi Smyth, the Astronomer Royal for

Scotland. During the voyage to Teneriffe (where, it will be remembered, his well-known Astronomer's Experiment was carried out), he tested the power of the gyroscope in giving steadiness, by causing a telescope to be so mounted, that the stand could not shift in position without changing the axial pose of a heavy rotating disc. The disc was set in rapid rotation by the sailors, and then the Professor directed the telescope towards a ship on the horizon. A fresh wind was blowing, so that everything on deck was swayed in lively sort by the tossing vessel; nor did the telescope *seem* a whit steadier—the motion of objects round it giving to the instrument an appearance of equal instability. But the officers were invited to look through the tube, and to their amazement, the distant ship was seen as steady in the middle of the telescopic field as though, instead of being set up on a tossing and rolling ship, the telescope had been mounted in an observatory on *terra firma*. The principle of the gyroscope has also been used for the purpose of so steadying the stand of a photographic camera placed in the car of a balloon, that photographs might be taken despite the tendency of the balloon to rotate. As applied to flying-machines, the gyroscope would require to be so modified in form that its weight would not prove an overload for the machine. This is practicable, because a flat horizontal disc, rotating rapidly, will support itself in the air if travelling horizontally forward with adequate swiftness. In other words, since travelling-machines *must* travel swiftly, the gyro-

scopic portion of the machine may be made to support itself.

It is this property of enforced rapidity of motion which renders the probable results of the mastery of our problem so important. It has been well remarked that two problems will be solved at once, when the first really successful flying-machine has been made—not only the problem of flight, but the problem of travelling more swiftly than by any contrivances yet devised. In the motion of a flying-machine, as distinguished from the flight of man by his own exertions, the swiftness of the bird's flight may be more than matched. It is a mere mechanical problem which has to be solved; and few mechanicians will deny that when once the true principles of flight have been recognised, the ingenuity of man is capable of constructing machines in which these principles shall be carried out. Iron and steam have given man the power of surpassing the speed of the swiftest of four-footed creatures—the horse, the greyhound, and the antelope. We have full confidence that the same useful servants place it in man's power to outvie in like manner the swiftest of winged creatures — the swallow, the pigeon, and the hawk.

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GAMBLING SUPERSTITIONS.

It might be supposed that those who are most familiar with the actual results which present themselves in long series of chance-games would form the most correct views respecting the conditions on which such results depend,—would be, in fact, freest from all superstitious ideas respecting chance or luck. The gambler who sees every system—his own infallible system included —foiled by the run of events, who witnesses the discomfiture of one gamester after another that for a time had seemed irresistibly lucky, and who can number by the hundred those who have been ruined by the love of play, might be expected to recognise the futility of all attempts to anticipate the results of chance combinations. It is, however, but too well known that the reverse is the case. The more familiar a man becomes with the multitude of such combinations, the more confidently he believes in the possibility of foretelling, —not, indeed, any special event, but the general run of several approaching events. There has never been a successful gambler who has not believed that his success (temporary though such success ever is, where games of pure chance are concerned) has been the result of skilful conduct on his own part; and there has never been a ruined gambler (though ruined gamblers are to be counted by thousands) who has not believed that when ruin overtook him he was on the very point of mastering the secret of success. It is this fatal confidence which gives to gambling its power of fascinating the lucky as

well as the unlucky. The winner continues to tempt fortune, believing all the while that he is exerting some special aptitude for games of chance, until the inevitable change of luck arrives; and thereafter he continues to play because he believes that his luck has only deserted him for a time, and must presently return. The unlucky gambler, on the contrary, regards his losses as sacrifices to ensure the ultimate success of his 'system,' and even when he has lost his all, continues firm in the belief that had he had more money to sacrifice he could have bound fortune to his side for ever.

I propose to consider some of the most common gambling superstitions,—noting, at the same time, that like superstitions prevail respecting chance events (or what is called fortune) even among those who never gamble.

Houdin, in his interesting book, *Les Tricheries des Grecs devoilées*, has given some amusing instances of the fruits of long gaming experience. 'They are presented,' says Steinmetz, from whose work, *The Gaming Table*, I quote them 'as the axioms of a professional gambler and cheat.' Thus we might expect that, however unsatisfactory to men of honest mind, they would at least savour of a certain sort of wisdom. Yet these axioms, the fruit of long study directed by self-interest, are all utterly untrustworthy.

'Every game of chance,' says this authority, 'presents two kinds of chances that are very distinct,—namely, those relating to the person interested, that is

the player; and those inherent in the combinations of the game.' That is, we are to distinguish between the chances proper to the game, and those depending on the luck of the player. Proceeding to consider the chances proper to the game itself, our friendly cheat sums them all up in two rules. First: 'Though chance can bring into the game all possible combinations, there are nevertheless certain limits at which it seems to stop: such, for instance, as a certain number turning up ten times in succession at roulette; this is possible, but it has never happened.' Secondly: 'In a game of chance, the oftener the same combination has occurred in succession, the nearer we are to the certainty that it will not recur at the next cast or turn up. This is the most elementary of the theories on probabilities; it is termed *the maturity of the chances*' (and he might have added that the belief in this elementary theory had ruined thousands). 'Hence,' he proceeds, 'a player must come to the table not only "in luck," but he must not risk his money except at the instant prescribed by the rules of the maturity of the chances.' Then follow the precepts for personal conduct:—'For gaming prefer roulette, because it presents several ways of staking your money—which permits the study of several. A player should approach the gaming-table perfectly calm and cool—just as a merchant or tradesman in treaty about any affair. If he gets into a passion it is all over with prudence, all over with good luck—for the demon of bad luck invariably pursues a passionate player. Every man who finds a pleasure in playing

runs the risk of losing.* A prudent player, before undertaking anything should put himself to the test to discover if he is "in vein" or in luck. In all doubt he should abstain. There are several persons who are constantly pursued by bad luck: to such I say—*never play*. Stubbornness at play is ruin. Remember that Fortune does not like people to be overjoyed at her favours, and that she prepares bitter deceptions for the imprudent who are intoxicated by success. Lastly, before risking your money at play, study your "vein," and the different probabilities of the game—termed, as aforesaid, the maturity of the chances.'

Before proceeding to exhibit the fallacy of the principles here enunciated—principles which have worked incalculable mischief—it may be well for us to sketch the history of the scamp who enunciated them,—so far, at least, as his gambling successes are concerned. His first meeting with Houdin took place at a subscription ball, where he managed to fleece Houdin 'and others to a considerable amount, contriving a dexterous escape when detected. Houdin afterwards fell in with him at Spa, where he found the gambler in the greatest poverty, and lent him a small sum—to practise his grand theories.' This sum the gambler lost, and Houdin advised him 'to take up a less dangerous occupation.' It was on this occasion, it would seem, that the gambler revealed to Houdin the particulars re-

* This *naïve* admission would appear, as we shall presently see, to have been the fruit of genuine experience on our gambler's part: it only requires that, for the words 'runs the risk,' we should read 'incurs the certainty' to be incontrovertible.

corded in his book. 'A year afterwards Houdin unexpectedly fell in with him again; but this time the fellow was transformed into what is called a "*demi-millionaire*," having succeeded to a large fortune on the death of his brother, who died intestate. According to Houdin, the following was the man's declaration at the auspicious meeting:—"I have," he said, "completely renounced gaming; I am rich enough; and care no longer for fortune. And yet," he added proudly, "if I now cared for the thing, how I could break those bloated banks in their pride, and what a glorious vengeance I could take of bad luck and its inflexible agents! But my heart is too full of my happiness to allow the smallest place for the desire of vengeance."' Three years later he died; and Houdin informs us that he left the whole of his fortune to various charitable institutions, his career after his acquisition of wealth going far to demonstrate the justice of Becky Sharp's theory, that it is easy to be honest on five thousand a year.

It is remarkable that the principles enunciated above are not merely erroneous, but self-contradictory. Yet it is to be noticed that though they are presented as the outcome of a life of gambling experiences, they are in reality entertained by all gamblers, however limited their experience, as well as by many who are only prevented by the lack of opportunity from entering the dangerous path which has led so many to ruin. These contradictory superstitions may be called severally,—the gambler's belief in his own good luck,

and his faith in the turn of luck. When he is considering his own fortune he does not hesitate to believe that on the whole the Fates will favour him, though this belief implies in reality the *persistence* of favourable conditions. On the contrary, when he is considering the fortunes of others who are successful in their play against him, he does not doubt that their good luck will presently desert them, that is, he believes in the *non-persistence* of favourable conditions in their case.

Taking in their order the gambling superstitions which have been presented above, we have, first of all, to inquire what truth there is in the idea that there are limits beyond which pure chance has no power of introducing peculiar combinations. Let us consider this hypothesis in the light of actual experience. Mr. Steinmetz tells us that, in 1813, a Mr. Ogden wagered 1,000 guineas to one that 'seven' would not be thrown with a pair of dice ten successive times. The wager was accepted (though it was egregiously unfair), and strange to say his opponent threw 'seven' *nine times running*. At this point Mr. Ogden offered 470 guineas to be off the bet. But his opponent declined (though the price offered was far beyond the real value of his chance). He cast yet once more, and threw 'nine,' so that Mr. Ogden won his guinea.

Now here we have an instance of a most remarkable series of throws, the like of which has never been recorded before or since. Before those throws had been made, it might have been asserted that the throwing of nine successive 'sevens' with a pair of

dice was a circumstance which chance could never bring about, for experience was as much against such an event as it would seem to be against the turning up of a certain number ten successive times at *roulette*. Yet experience now shows that the thing is possible; and if we are to limit the action of chance, we must assert that the throwing of 'seven' *ten* times in succession is an event which will never happen. Yet such a conclusion obviously rests on as unstable a basis as the former, of which experience has disposed. Observe, however, how the two gamblers viewed this very eventuality. Nine successive 'sevens' had been thrown; and if there were any truth in the theory that the power of chance was limited, it might have been regarded as all but certain that the next throw would not be a 'seven.' But a run of bad fortune had so shaken Mr. Ogden's faith in his luck (as well as in the theory of the maturity of the chances) that he was ready to pay 470 guineas (nearly thrice the mathematical value of his opponent's chance) in order to save his endangered thousand; and so confident was his opponent that the run of luck would continue that he declined this very favourable offer. Experience had in fact shown both the players, that although 'sevens' could not be thrown for ever, yet there was no saying when the throw would change. Both reasoned probably that as an eighth throw had followed seven successive throws of 'seven' (a wonderful chance), and as a ninth had followed eight successive throws (an unprecedented event), a tenth might well follow the nine (though

hitherto no such series of throws had ever been heard of). They were forced as it were by the run of events to reason justly as to the possibility of a tenth throw of 'seven,'—nay, to exaggerate that possibility into probability; and it appears from the narrative that the strange series of throws quite checked the betting propensities of the bystanders, and that not one was led to lay the wager (which according to ordinary gambling superstitions would have been a safe one) that the tenth throw would not give 'seven.'

We have spoken of the unfairness of the original wager. It may interest our readers to know exactly how much should have been wagered against a single guinea, that ten 'sevens' would not be thrown. With a pair of dice there are thirty-six possible throws, and six of these give 'seven' as the total. Thus the chance of throwing 'seven' is one sixth, and the chance of throwing 'seven' ten times running is obtained by multiplying six into itself ten times, and placing the resulting number under unity, to represent the minute fractional chance required. It will be found that the number thus obtained is 60,466,176, and instead of 1,000 guineas, fairness required that 60,466,175 guineas should have been wagered against one guinea, so enormous are the chances against the occurrence of ten successive throws of 'seven.' Even against nine successive throws the fair odds would have been 10,077,595 to one, or about forty thousand guineas to a farthing. But when the nine throws of 'seven' had been made, the chance of a tenth throw of 'seven' was

simply one-sixth as at the first trial. If there were any truth in the theory of the 'maturity of the chances,' the chance of such a throw would of course be greatly diminished. But even taking the mathematical value of the chance, Mr. Ogden need in fairness only have offered a sixth part of 1,001 guineas (the amount of the stakes), or 166 Guas. 17*s.* 6*d.*, to be off his wager. So that his opponent accepted in the first instance an utterly unfair offer, and refused in the second instance a sum exceeding by more than three hundred guineas the real value of his chance.

Closely connected with the theory about the range of possibility in the matter of chance combinations, is the theory of the maturity of the chances,—'the most elementary of the theories on probabilities.' It might safely be termed the most mischievous of gambling superstitions.

As an illustration of the application of this theory, we may cite the case of an Englishman, once well known at foreign gambling-tables, who had based a system on a generalisation of this theory. In point of fact the theory asserts that when there has been a run in favour of any particular event, the chances in favour of the event are reduced, and therefore, necessarily, the chances in favour of other events are increased. Now our Englishman watched the play at the *roulette* table for two full hours, carefully noting the numbers which came up during that time. Then, eschewing those numbers which had come up oftenest, he staked his money on those which had come up very seldom or

not at all. Here was an infallible system according to 'the most elementary of the theories of probability.' The tendency of chance-results to right themselves, so that events equally likely in the first instance will occur an equal number of times in the long run, was called into action to enrich our gambler and to ruin the unlucky bankers. Be it noted, in passing, that events do thus right themselves, though this circumstance does not operate quite as the gambler supposed, and cannot be trusted to put a penny into any one's pocket. The system was tried, however, and instead of reasoning respecting its soundness, we may content ourselves with recording the result. On the first day our Englishman won more than seven hundred pounds in a single hour. 'His exultation was boundless. He thought he had really discovered the "philosopher's stone." Off he went to his bankers, and transmitted the greater portion of his winnings to London. The next day he played and lost fifty pounds; and the following day he achieved the same result, and had to write to town for remittances. In fine, in a week he had lost all the money he won at first, with the exception of fifty pounds, which he reserved to take him home; and being thoroughly convinced of the exceeding fickleness of fortune, he has never staked a sixpence since, and does all in his power to dissuade others from playing.' *

It may appear paradoxical to say, that chance re-

* From an interesting paper entitled '*Le Jeu est fait*,' in *Chambers's Journal*.

sults right themselves—nay, that there is an absolute certainty that in the long run they will occur as often (in proportion) as their respective chances warrant, and at the same time to assert that it is utterly useless for any gambler to trust to this circumstance. Yet not only is each statement true, but it is of first-rate importance in the study of our subject that the truth of each should be clearly recognised.

That the first statement is true, will perhaps not be questioned. The reasoning on which it is based would be too abstruse for these pages; but it has been experimentally verified over and over again. Thus, if a coin be tossed many thousands of times, and the numbers of resulting 'heads' and 'tails' be noted, it is found, not necessarily that these numbers differ from each other by a very small quantity, but that their difference is small compared with either. In mathematical phrase, the two numbers are nearly in a ratio of equality. Again, if a die be tossed, say, six million times, then, although there will not probably have been exactly a million throws of each face, yet the number of throws of each face will differ from a million by a quantity very small indeed compared with the total number of throws. So certain is this law, that it has been made the means of determining the real chances of an event, or of ascertaining facts which had been before unknown. Thus, De Morgan relates the following story in illustration of this law. He received it 'from a distinguished naval officer, who was once employed to bring home a cargo of dollars.' 'At the end of the voyage,' he says, 'it

was discovered that one of the boxes which contained them had been forced; and on making further search a large bag of dollars was discovered in the possession of some one on board. The coins in the different boxes were a mixture of all manner of dates and sovereigns; and it occurred to the commander, that if the contents of the boxes were sorted, a comparison of the proportions of the different sorts in the bag, with those in the box which had been opened, would afford strong presumptive evidence one way or the other. This comparison was accordingly made, and the agreement between the distribution of the several coins in the bag and those in the box was such as to leave no doubt as to the former having formed a part of the latter.' If the bag of stolen dollars had been a small one the inference would have been unsafe, but the great number of the dollars corresponded to a great number of chance trials; and as in such a large series of trials the several results would be sure to occur in numbers corresponding to their individual chances, it followed that the number of coins of the different kinds in the stolen lot would be proportional, or very nearly so, to the number of those respective coins in the forced box. Thus, in this case the thief increased the strength of the evidence against him by every dollar he added to his ill-gotten store.

We may mention, in passing, an even more curious application of this law, to no less a question than that much-talked of but little understood problem, the squaring of the circle. It can be shown by mathe-

matical reasoning, that, if a straight rod be so tossed at random into the air as to fall on a grating of equidistant parallel bars, the chance of the rod falling through depends on the length and thickness of the rod, the distance between the parallel bars, *and* the proportion in which the circumference of a circle exceeds the diameter. So that when the rod and grating have been carefully measured, it is only necessary to know the proportion just mentioned in order to calculate the chance of the rod falling through. But also, if we can learn in some other way the chance of the rod falling through, we can infer the proportion referred to. Now the law we are considering teaches us that if we only toss the rod often enough, the chance of its falling through will be indicated by the number of times it actually does fall through, compared with the total number of trials. Hence we can estimate the proportion in which the circumference of a circle exceeds the diameter by merely tossing a rod over a grating several thousand times, and counting how often it falls through. The experiment has been tried, and Professor De Morgan tells us that a very excellent evaluation of the celebrated proportion (the determination of which is equivalent in reality to squaring the circle) was the result.

And let it be noticed, in passing, that this inexorable law—for in its effects it is the most inflexible of all the laws of probability—shows how fatal it must be to contend long at any game of pure chance, where the odds are in favour of our opponent. For instance, let us assume for a moment that the assertion of the foreign

gaming bankers is true, and that the chances are but from 1¼ to 2½ per cent. in their favour. Yet in the long run, this percentage must manifest its effects. Where a few hundreds have been wagered the bank may not win 1¼ or 2½ on each, or may lose considerably; but where thousands of hundreds are wagered, the bank will certainly win about their percentage, and the players will therefore lose to a corresponding extent. This is inevitable, so only that the play continue long enough. Now, it is sometimes forgotten that to ensure such gain to the bank, it is by no means necessary that the players should come prepared to stake so many hundreds of pounds. Those who sit down to play may not have a tithe of the sum necessary—if only wagered once—to ensure the success of the bank. But every florin the players bring with them may be, and commonly is, wagered over and over again. There is repeated gain and loss, and loss and gain; insomuch that the player who finally loses a hundred pounds, may have wagered in the course of the sitting a thousand or even many thousand pounds. Those fortunate beings who 'break the bank' from time to time, may even have accomplished the feat of wagering millions during the process which ends in the final loss of the few thousands they may have begun with.

Why is it, then, it will be asked, that this inexorable law is yet not to be trusted? For this reason, simply, that the mode of its operation is altogether uncertain. If in a thousand trials there has been a remarkable preponderance of any particular class of events, it is

not a whit more probable that the preponderance will be compensated by a corresponding deficiency in the next thousand trials than that it will be repeated in that set also. The most probable result of the second thousand trials is precisely that result which was most probable for the first thousand—that is, that there will be no marked preponderance either way. But there *may be* such a preponderance; and it may lie either way. It is the same with the next thousand, and the next, and for every such set. They are in no way affected by preceding events. In the nature of things, how can they be? But, 'the whirligig of time brings in its revenges' in its own way. The balance is restored just as chance directs. It may be in the next thousand trials, it may be not before many thousands of trials. We are utterly unable to guess when or how it will be brought about.

But it may be urged that this is mere assertion; and many will be very ready to believe that it is opposed to experience, or even contrary to common sense. Yet experience has over and over again confirmed the matter, and common sense, though it may not avail to unravel the seeming paradox, yet cannot insist on the absurdity that coming events of pure chance are affected by completed events of the same kind. If a person has tossed 'heads' nine times running (we assume fair and lofty tosses with a well-balanced coin), common sense teaches him, as he is about to make the tenth trial, that the chances on that trial are precisely the same as the chances on the first. It would, indeed,

have been rash for him to predict that he would reach that trial without once failing to toss 'head;' but as the thing has happened, the odds originally against it count for nothing. They are disposed of by known facts. We have said, however, that experience confirms our theory. It chances that a series of experiments have been made on coin-tossing. Buffon was the experimenter, and he tossed thousands of times, noting always how many times he tossed 'head' running before 'tail' appeared. In the course of these trials he many times tossed 'head' nine times running. Now, if the tossing 'head' nine times running rendered the chance of tossing a tenth head much less than usual, it would necessarily follow that in considerably more than one half of these instances Buffon would have failed to toss a tenth head. But he did not. I forget the exact numbers, but this I know, that in about half the cases in which he tossed nine 'heads' running, the next trial also gave him 'head;' and about half of these tossings of ten successive 'heads' were followed by the tossing of an eleventh 'head.' In the nature of things this was to be expected.

And now let us consider the cognate questions suggested by our sharper's ideas respecting the person who plays. This person is to consider carefully whether he is '*in vein*,' and not otherwise to play. He is to be cool and businesslike, for fortune is invariably adverse to an angry player. Steinmetz, who appears to place some degree of reliance on the suggestion that a player should be 'in vein,' cites in illustration and confirma-

tion of the rule the following instance from his own experience:—'I remember,' he says, 'a curious incident in my childhood which seems very much to the point of this axiom. A magnificent gold watch and chain were given towards the building of a church, and my mother took three chances, which were at a very high figure, the watch and chain being valued at more than 100*l*. One of these chances was entered in my name, one in my brother's, and a third in my mother's. I had to throw for her as well as myself. My brother threw an insignificant figure; for myself I did the same; but, oddly enough, I refused to throw for my mother on finding that I had lost my chance, saying that I should wait a little longer—rather a curious piece of prudence' (read, rather, superstition) 'for a child of thirteen. The raffle was with three dice; the majority of the chances had been thrown, and thirty-four was the highest.' (It is to be presumed that the three dice were thrown twice, yet 'thirty-four' is a remarkable throw with six dice, and 'thirty-six' altogether exceptional.) 'I went on throwing the dice for amusement, and was surprised to find that every throw was better than the one I had in the raffle. I thereupon said, "Now I'll throw for mamma." I threw thirty-six, which won the watch! My mother had been a large subscriber to the building of the church, and the priest said that my winning the watch for her was quite *providential*. According to M. Houdin's authority, however, it seems that I only got into "vein,"—but how I came to pause and defer throwing the last

chance has always puzzled me respecting this incident of childhood, which made too great an impression ever to be effaced.'

It is probable that most of our readers can recall some circumstance in their lives, some surprising coincidence, which has caused a similar impression, and which they have found it almost impossible to regard as strictly fortuitous.

In chance games especially, curious coincidences of the sort occur, and lead to the superstitious notion that they are not mere coincidences, but in some definite way associated with the fate or fortune of the player, or else with some event which has previously taken place,—a change of seats, a new deal, or the like. There is scarcely a gambler who is not prepared to assert his faith in certain observances whereby, as he believes, a change of luck may be brought about. In an old work on card-games the player is gravely advised, if the luck has been against him, to turn three times round with his chair, 'for then the luck will infallibly change in your favour.'

Equally superstitious is the notion that anger brings bad luck, or, as M. Houdin's authority puts it, that 'the demon of bad luck invariably pursues a passionate player.' At a game of pure chance good temper makes the player careless under ill-fortune, but it cannot secure him against it. In like manner, passion may excite the attention of others to the player's losses, and in any case causes himself to suffer more keenly under them, but it is only in this sense that passion is un-

lucky for him. He is as likely to make a lucky hit when in a rage as in the calmest mood.

It is easy to see how superstitions such as these take their origin. We can understand that since one who has been very unlucky in games of pure chance, is not antecedently likely to continue equally unlucky, a superstitious observance is not unlikely to be followed by a seeming change of luck. When this happens the coincidence is noted and remembered; but failures are readily forgotten. Again, if the fortunes of a passionate player be recorded by dispassionate bystanders, he will not appear to be pursued by worse luck than his neighbours; but he will be disposed to regard himself as the victim of unusual ill-fortune. He may perhaps register a vow to keep his temper in future; and then his luck may seem to him to improve, even though a careful record of his gains and losses would show no change whatever in his fortunes.

But it may not seem quite so easy to explain those undoubted runs of luck, by which players 'in the vein' (as supposed) have broken gaming-banks, and have enabled those who have followed their fortunes to achieve temporary success. The history of the notorious Garcia, and of others who like him have been for awhile the favourites of fortune, will occur at once to many of my readers, and will appear to afford convincing proof of the theory that the luck of such gamesters has had a real influence on the fortunes of the game. The following narrative gives an accurate and graphic picture of the way in which these 'bank-

breakers' are followed and believed in, while their success seems to last.

The scene is laid in one of the most celebrated German Kursaals.

'What a sudden influx of people into the room! Now, indeed, we shall see a celebrity. The tall light-haired young man coming towards us, and attended by such a retinue, is a young Saxon nobleman who made his appearance here a short time ago, and commenced his gambling career by staking very small sums; but, by the most extraordinary luck, he was able to increase his capital to such an extent that he now rarely stakes under the maximum, and almost always wins. They say that when the croupiers see him place his money on the table, they immediately prepare to pay him, without waiting to see which colour has actually won, and that they have offered him a handsome sum down to desist from playing while he remains here. Crowds of people stand outside the Kursaal doors every morning, awaiting his arrival, and when he comes following him into the room, and staking as he stakes. When he ceases playing they accompany him to the door, and shower on him congratulations and thanks for the good fortune he has brought them. See how all the people make way for him at the table, and how deferential are the subdued greetings of his acquaintances! He does not bring much money with him, his luck is too great to require it. He takes some notes out of a case, and places maximums on *black* and *couleur*. A crowd of eager hands are immediately out-

stretched from all parts of the table, heaping up silver and gold and notes on the spaces on which he has staked his money, till there scarcely seems room for another coin, while the other spaces on the table only contain a few florins staked by sceptics who refuse to believe in the count's luck.' He wins; and the narrative proceeds to describe his continued successes, until he rises from the table a winner of about one hundred thousand francs at that sitting.

The success of Garcia was so remarkable at times as to affect the value of the shares in the *Privilegirte Bank* ten or twenty per cent. Nor would it be difficult to cite many instances which seem to supply incontrovertible evidence that there is something more than common chance in the temporary successes of these (so-called) fortunate men.

Indeed, to assert merely that in the nature of things there can be no such thing as luck that can be depended on even for a short time, would probably be quite useless. There is only one way of meeting the infatuation of those who trust in the fates of lucky gamesters. We can show that, granted a sufficient number of trials—and it will be remembered that the number of those who have risked their fortunes at *roulette* and *rouge et noir* is incalculably great—there must *inevitably* be a certain number who appear exceptionally lucky; or, rather, that the odds are overwhelmingly against the continuance of play on the scale which prevails at the foreign gambling tables, without the occurrence of several instances of persistent runs of luck.

To remove from the question the perplexities resulting from the nature of the above-named games, let us suppose that the tossing of a coin is to determine the success or failure of the player, and that he will win if he throws 'head.' Now if a player tossed 'head' twenty times running on any occasion it would be regarded as a most remarkable run of luck, and it would not be easy to persuade those who witnessed the occurrence that the thrower was not in some special and definite manner the favourite of Fortune. We may take such exceptional success as corresponding to the good fortune of a 'bank-breaker.' Yet it is easily shown that with a number of trials which must fall enormously short of the number of cases in which fortune is risked at foreign Kursaals, the throwing of twenty successive 'heads' would be practically *ensured.* Suppose every adult person in Britain—say 10,000,000 persons in all—were to toss a coin, each tossing until 'tail' was thrown; then it is practically certain that several among them would toss twenty times before 'tail' was thrown. Thus: It is certain that about five millions would toss 'head' once; of these about one-half, or some two millions and a half, would toss 'head' on the second trial; about a million and a quarter would toss 'head' on the third trial; about six hundred thousand on the fourth; some three hundred thousand on the fifth; and by proceeding in this way—roughly halving the numbers successively obtained—we find that some eight or nine of the ten million persons would be almost certain to toss 'head' twenty times

running. It must be remembered that so long as the numbers continue large the probability that *about* half will toss 'head' at the next trial amounts almost to a certainty. For example, about 140 toss 'head' sixteen times running: now, it is utterly unlikely that of these 140, fewer than sixty will toss 'head' yet a seventeenth time. But if the above process failed on trial to give even one person who tossed 'heads' twenty times running—an utterly improbable event—yet the trial could be made four or five times, with practical certainty that not one or two, but thirty or forty, persons would achieve the seemingly incredible feat of tossing 'head' twenty times running. Nor would all these thirty or forty persons fail to throw even three or four more 'heads.'

Now, if we consider the immense number of trials made at gambling-tables, and if we further consider the gamblers as in a sense typified by our ten millions of coin-tossers, we shall see that it is not merely probable but absolutely certain that from time to time there must be marvellous runs of luck at *roulette*, *rouge et noir*, *hazard*, *faro*, and other games of chance. Suppose that at the public gaming-tables on the Continent there sit down each night but one thousand persons in all, that each person makes but ten ventures each night, and that there are but one hundred gambling nights in the year—each supposition falling far below the truth—there are then one million ventures each year. It cannot be regarded as wonderful, then, that among the fifty millions of

ventures made (on this supposition) during the last half century, there should be noted some runs of luck which on any single trial would seem incredible. On the contrary, this is so far from being wonderful that it would be far more wonderful if no such runs of luck had occurred. It is probable that if the actual number of ventures, and the circumstances of each, could be ascertained, and if any mathematician could deal with the tremendous array of figures in such sort as to deduce the exact mathematical chance of the occurrence of bank-breaking runs of luck, it would be found that the antecedent odds were many millions to one in favour of the occurrence of a certain number of such events. In the simpler case of our coin-tossers the chance of twenty successive 'heads' being tossed can be quite readily calculated. I have made the calculation, and I find that if the ten million persons had each two trials the odds would be more than 10,000 to 1 in favour of the occurrence of twenty successive 'heads' once at least; and only a million and a half need have a single trial each, in order to give an even chance of such an occurrence.

But we may learn a further lesson from our illustrative tossers. We have seen that granted only a sufficient number of trials, runs of luck are practically certain to occur: but we may also infer that no run of luck can be *trusted* to continue. The very principle which has led us to the conclusion that several of our tossers would throw twenty 'heads' successively, leads also to the conclusion that one who has tossed 'heads'

twelve or thirteen times, or any other considerable number of times in succession, is not more (or less) likely to toss 'head' on the next trial than at the beginning. *About half*, we said, in discussing the fortunes of the tossers, would toss 'head' at the next trial: in other words, *about half* would fail to toss 'head.' The chances for and against these lucky tossers are equal at the next trial, precisely as the chances for and against the least lucky of the ten million tossers would be equal at any single tossing.

Yet, it may be urged, experience shows that luck continues; for many have won by following the lead of lucky players. Now I might, at the outset, point out that this belief in the continuance of luck is suggested by an idea directly contradictory to that on which is based the theory of the maturity of the chances. If the oftener an event has occurred, the more unlikely is its occurrence at the next trial—the common belief—then contrary to the common belief, the oftener a player has won (that is, the longer has been his run of luck), the more unlikely is he to win at the next venture. We cannot separate the two theories, and assume that the theory of the maturity of the chances relates to the play, and the theory of runs of luck to the player. The success of the player at any trial is as distinctly an event—a chance event—as the turning up of ace or deuce at the cast of a die.

What then are we to say of the experience of those who have won money by following a lucky player?

Let us revert to our coin-tossers. Let us suppose that the progress of the venture in a given county is made known to a set of betting men in that county; and that when it becomes known that a person has tossed 'head' twelve times running, the betting men hasten to back the luck of that person. Further, suppose this to happen in every county in England. Now we have seen that these persons are no more likely to toss a thirteenth 'head,' than they are to fail. About half will succeed and about half will fail. Thus about half their backers will win and about half will lose. But the successes of the winners will be widely announced; while the mischances of the losers will be concealed. This will happen—the like notoriously does happen—for two reasons. First, gamblers pay little attention to the misfortunes of their fellows: the professed gambler is utterly selfish, and moreover he hates the sight of misfortune because it unpleasantly reminds him of his own risks. Secondly, losing gamblers do not like their losses to be noised abroad; they object to having their luck suspected by others, and they are even disposed to blind themselves to their own ill-fortune as far as possible. Thus, the inevitable success of about one half of our coin-tossers would be accompanied inevitably by the success of those who 'backed their luck,' and the successes of such backers would be bruited abroad and be quoted as examples; while the failure of those who had backed the other half (whose luck was about to fail them), would be comparatively unnoticed. Unquestionably the like

holds in the case of public gambling-tables. If any doubt this, let them inquire what has been heard of those who continued to back Garcia and other 'bank-breakers.' We know that Garcia and the rest of these lucky gamblers have been ruined; they had risen too high and were followed too constantly for their fall to remain unnoticed. But what has been heard of those unfortunates who backed Garcia after his last successful venture, and before the change in his luck had been made manifest? We hear nothing of them, though a thousand stories are told of those who made money while Garcia and the rest were 'in luck.'

In passing, we may add to these considerations the circumstance that it is the interest of gaming-bankers to conceal the misfortunes of the unlucky, and to announce and exaggerate the success of the fortunate.

I by no means question, be it understood, the possibility that money may be gained quite safely by gambling. Granting, first, odds such as the 'banks' have in their favour; secondly, a sufficient capital to prevent premature collapse; and thirdly, a sufficient number of customers, success is absolutely certain in the long run. The capital of the gambling-public doubtless exceeds collectively the capital of the gambling-banks; but it is not used collectively: the fortunes of the gambling-public are devoured successively, the sticks which would be irresistible when combined, are broken one by one. We leave our readers to judge whether this circumstance should encourage gambling or the reverse.

It is also easy to understand why in the betting on horse-racing in this country and others, success ordinarily attends the professional bettor, rather than the amateur,—or, in the slang of the subject, why 'the ring' gets the advantage of 'the gentlemen?' Apart from his access to secret sources of information, the professional bettor nearly always 'lays the odds,' that is, bets against individual horses; while the amateur 'takes the odds,' or backs the horse he fancies. Now, if the odds represented the strict value of the horse's chance, it would be as safe in the long run to 'take' as to 'lay' the odds. But no professional bettor lays fair odds, save by mistake. Nor is it difficult to get the amateur to take unfair odds. For 'backing' is seemingly a safe course. The 'backer' risks a small sum to gain a large one, and if the fair large sum is a a little reduced, he still conceives that he is not risking much. Yet (to take an example), if the true odds are nine to one against a horse, and the amateur sportsman consents to take eight to one in hundreds, then, though he risks but a single hundred against the chance of winning eight, he has been as truly swindled out of ten pounds as though his pocket had been picked of that sum. This is easily shown. The total sum staked is nine hundred pounds, and at the odds of nine to one, the stakes should have been respectively ninety pounds and eight hundred and ten pounds. Our amateur should, therefore, only have risked ninety pounds for his fair chance of the total sum staked. But he has been persuaded to risk one hundred pounds for

that chance. He has therefore been swindled out of ten pounds. And in the long run, if he laid several hundreds of wagers of the same amount, and on the same plan, he would inevitably lose on the average about ten pounds per venture.

In conclusion, I may thus present the position of the gambler who is not ready to secure Fortune as his ally by trickery:—If he meets gamblers who are not equally honest, he is not trying his luck against theirs, but, at the best (as De Morgan puts it) only a part of his against more than the whole of theirs: if he meets players as honest as himself he must, nevertheless, as Lord Holland said to Selwyn, 'be—in earnest and without irony—*en verité le serviteur très humble des événements*, in truth the very humble servant of events.'

(From the *Cornhill Magazine* for June 1872.)

COINCIDENCES AND SUPERSTITIONS.

Every one is familiar with the occasional occurrence of coincidences, so strange—considered abstractly—that it appears difficult to regard them as due to mere casualty. The mind is dwelling on some person or event, and suddenly a circumstance happens which is associated in some altogether unexpected, and as it were improbable, manner with that person or event. A scheme has been devised which can only fail if some utterly unlikely series of events should occur, and pre-

cisely those events take place. Sometimes a coincidence is utterly trivial, yet attracts attention by the singular improbability of the observed events. We are thinking of some circumstance, let us say, in which two or three persons are concerned, and the first book or paper we turn to, shows, in the very first line we look at, the names of those very persons, though really relating to others in no way connected with them; and so on, with many other kinds of coincidence, equally trivial and equally singular. Yet again, there are other coincidences which are rendered striking by their frequent recurrence. It is to such recurring coincidences that common superstitions owe their origin, while the special superstitions thus arising (that is, superstitions entertained by individuals) are innumerable. It is lucky to do this, unlucky to do that, say those who believe in common superstitions; and they can always cite many coincidences in favour of their opinion. But it is amazing how common are the private superstitions entertained by many who smile at the superstitions of the ignorant: we must suppose that all such superstitions have been based upon observed coincidences. Again, there are tricks or habits which have obviously had their origin in private superstitions. Dr. Johnson may not have believed that some misfortune would happen to him if he failed to place his hand on every post which he passed along a certain route; he would certainly not have maintained such an opinion publicly: yet in the first instance that habit of his must have had its origin in

some observed coincidences; and when once a habit of the sort is associated with the idea of good luck, even the strongest minds have been found unready to shake off the superstition.

It is to be noticed, indeed, that many who reject the idea that the ordinary superstitions have any real significance, are nevertheless unwilling to run directly counter to them. Thus, a man shall be altogether sceptical as to the evil effects which follow, according to a common superstition, from passing under a ladder; he may be perfectly satisfied that the proper reason for not passing under a ladder is the possibility of its falling, or of something falling from it: yet he will not pass under a ladder, even though it is well secured, and obviously carries nothing which can fall upon him. So with the old superstition, that a broken mirror brings seven years of sorrow, which, according to some, dates from the time when a mirror was so costly as to represent seven years' savings,—there are those who despise the superstition who would yet be unwilling to tempt fate (as they put it) by wilfully breaking even the most worthless old looking-glass. A story is not unfrequently quoted in defence of such caution. Every one knows that sailors consider it unlucky for a ship to sail on a Friday. A person, anxious to destroy this superstition, had a ship's keel laid on a Friday, the ship launched on a Friday, her masts taken in from the sheer-hulk on a Friday, the cargo shipped on a Friday; he found (heaven knows how, but so the story runs) a Captain Friday to command her; and

lastly, she sailed on a Friday. But the superstition was not destroyed, for the ship never returned to port, nor was the manner of her destruction known. Other instances of the kind might be cited. Thus a feeling is entertained by many persons not otherwise superstitious, that bad luck will follow any wilful attempt to run counter to a superstition.

It is somewhat singular that attempts to correct even the more degrading forms of superstition have often been as unsuccessful as those attempts which may perhaps not unfairly be called tempting fate. Let me be understood. To refer to the example already given, it is a manifest absurdity to suppose that the sailing of a ship on a Friday is unfortunate; and it would be a piece of egregious folly to consider such a superstition when one has occasion to take a journey. But the case is different when any one undertakes to *prove* that the superstition is an absurdity; simply because he must assume in the first instance that he will succeed, a result which cannot be certain; and such confidence, apart from all question of superstition, is a mistake. In fact, a person so acting errs in the very same way as those whom he wishes to correct; *they* refrain from a certain act because of a blind fear of bad luck, and *he* proceeds to the act with an equally blind belief in good luck.

But one cannot recognise the same objection in the case of a person who tries to correct some superstition by actions not involving any tempting of fortune. Yet it has not unfrequently happened that such actions

have resulted in confirming the superstition. The following instance may be cited. An old woman came to Flamsteed, the first Astronomer Royal, to ask him whereabouts a certain bundle of linen might be, which she had lost. Flamsteed determined to show the folly of that belief in astrology which had led her to Greenwich Observatory (under some misapprehension as to the duties of an Astronomer Royal). He 'drew a circle, put a square into it, and gravely pointed out a ditch, near her cottage, in which he said it would be found.' He then waited until she should come back disappointed, and in a fit frame of mind to receive the rebuke he intended for her; but 'she came back in great delight, with the bundle in her hand, found in the very place.'

In connection with this story, though bearing rather on over-hasty scientific theorizing than on ordinary superstitions, I quote the following story from De Morgan's *Budget of Paradoxes*: 'The late Baron Zach received a letter from Pons, a successful finder of comets, complaining that for a certain period he had found no comets, though he had searched diligently. Zach, a man of much sly humour, told him that no spots had been seen on the sun for about the same time—which was true—and assured him that when the spots came back the comets would come with them. Some time after he got a letter from Pons, who informed him with great satisfaction that he was quite right; that very large spots had appeared on the sun, and that he had found a comet shortly after. I have the story in

Zach's handwriting. It would mend the story exceedingly if some day a real relation should be established between comets and solar spots. Of late years good reason has been shown for advancing a connection between these spots and the earth's magnetism. If the two things had been put to Zach he would probably have chosen the comets. Here is a hint for a paradox: the solar spots are the dead comets, which have parted with their light and heat to feed the sun, as was once suggested. I should not wonder if I were too late, and the thing had been actually maintained.' De Morgan was not far wrong. Something very like his paradox was advocated, before the Royal Astronomical Society, by Commander Ashe, of Canada, earlier we believe than the date of De Morgan's remarks. I happen to have striking evidence in favour of De Morgan's opinion about the view which Zach would probably have formed of the theory which connects sun-spots and the earth's magnetism. When the theory was as yet quite new, I referred to it in a company of Cambridge men, mostly high mathematicians, and it was received at first as an excellent joke, and welcomed with laughter. It need hardly be said, however, that when the nature of the evidence was stated, the matter assumed another aspect. Yet it may be mentiond, in passing, that there are those who maintain that, after all, this theory is untrue, the evidence on which it rests being due only to certain strange coincidences.

In many instances, indeed, considerable care is re-

quired to determine whether real association or mere casual coincidence is in question. It is surprising how, in some cases, an association can be traced between events seemingly in no way connected. One is reminded of certain cases of derivation. Ninety-nine persons out of a hundred, for instance, would laugh at the notion that the words 'hand' and 'prize' are connected; yet the connection is seen clearly enough when 'prize' is traced back to 'prehendo,' with the root 'hend' obviously related to 'hand,' 'hound,' and so on. Equally absurd at a first view is the old joke that the Goodwin Sands were due to the building of a certain church; yet if moneys which had been devoted to the annual removal of the gathering sand were employed to defray the cost of the church, mischief, afterwards irreparable, might very well have been occasioned. Even the explanation of certain mischances as due to the circumstance that 'there was no weathercock at Kiloe,' may admit of a not quite unreasonable interpretation. I leave this as an exercise for the ingenious reader.

But when we have undoubted cases of coincidence, without the possibility of any real association (setting the supernatural aside), we have a problem of some interest to deal with. To explain them as due to some special miraculous intervention may be satisfactory to many minds, in certain cases; but in others it is impossible to conceive that the matter has seemed worthy of a miracle. Even viewing the question in its bearing on religious ideas, there are cases where it seems far more mischievous (as bringing ridicule on

the very conception of the miraculous) to believe in supernatural intervention, than to reject such an explanation on the score of antecedent improbability. Horace's rule, '*Nec deus intersit nisi dignus vindice nodus*,' remains sound when we write '*Deus*' for '*deus*.'

Now there have been cases so remarkable, yet so obviously unworthy of supernatural intervention, that we are perplexed to find any reasonable explanation of the matter. The following, adduced by De Morgan, will, I have no doubt, recall corresponding cases in the experience of readers of these lines:—'In the summer of 1865,' he says, 'I made myself first acquainted with the tales of Nathaniel Hawthorne, and the first I read was about the siege of Boston in the War of Independence. I could not make it out: everybody seemed to have got into somebody else's place. I was beginning the second tale, when a parcel arrived: it was a lot of odd pamphlets and other rubbish, as he called it, sent by a friend who had lately sold his books, had not thought it worth while to send these things for sale, but thought I might like to look at them, and possibly keep some. The first thing I looked at was a sheet, which, being opened, displayed "A plan of Boston and its environs, showing the true situation of his Majesty's army, and also that of the rebels, drawn by an engineer, at Boston, October 1775." Such detailed plans of current sieges being then uncommon, it is explained that "The principal part of this plan was surveyed by Richard Williams, Lieu-

tenant, at Boston; and sent over by the son of a nobleman to his father in town, by whose permission it was published." I immediately saw that my confusion arose from my supposing that the king's troops were besieging the rebels, when it was just the other way' (a mistake, by the way, which does not suggest that the narrative was particularly lucid).

Another instance cited by De Morgan is yet more remarkable, though it is not nearly so strange as a circumstance which I shall relate afterwards:—'In August, 1861,' he says, 'M. Senarmont, of the French Institute, wrote to me to the effect that Fresnel had sent to England in, or shortly after, 1824, a paper for translation and insertion in the *European Review*, which shortly after expired. The question was what had become of the paper. I examined the *Review* at the Museum, found no trace of the paper, and wrote back to that effect, at the Museum, adding that everything now depended on ascertaining the name of the editor, and tracing his papers: of this I thought there was no chance. I posted the letter on my way home, at a post-office in the Hampstead Road, at the junction with Edward Street, on the opposite side of which is a bookstall. Lounging for a moment over the exposed books, *sicut meus est mos*, I saw within a few moments of the posting of the letter, a little catchpenny book of anecdotes of Macaulay, which I bought, and ran over for a minute. My eye was soon caught by this sentence: 'One of the young fellows immediately wrote to the Editor (Mr. Walker) of the *European Review*.' I thus

got the clue by which I ascertained that there was no chance of recovering Fresnel's papers. Of the mention of current Reviews not one in a thousand names the editor.' It will be noticed that there was a double coincidence in this case. It was sufficiently remarkable that the first mention of a review, after the difficulty had been recognised, should relate to the *European*, and give the name of the editor; but it was even more remarkable that the occurrence should be timed so strangely as was actually the case.

But the circumstance I am now to relate, seems to me to surpass in strangeness all the coincidences I have ever heard of. It relates to a matter of considerable interest apart from the coincidence.

When Dr. Thomas Young was endeavouring to interpret the inscription of the famous Rosetta Stone, Mr. Grey (afterwards Sir George Francis Grey) was led on his return from Egypt to place in Young's hands some of the most valuable fruits of his researches among the relics of Egyptian art, including several fine specimens of writing on papyrus, which he had purchased from an Arab at Thebes, in 1820. Before these had reached Young, a man named Casati had arrived in Paris, bringing with him from Egypt a parcel of Egyptian manuscripts, among which Champollion observed one which bore in its preamble some resemblance to the text of the Rosetta Stone. This discovery attracted much attention; and Dr. Young having procured a copy of the papyrus, attempted to decipher and translate it. He had made some pro-

gress with the work when Mr. Grey gave him the new papyri. 'These,' says Dr. Young, 'contained several fine specimens of writing and drawing on papyrus; they were chiefly in hieroglyphics and of a mythological nature; but two which he had before described to me, as particularly deserving attention, and which were brought, through his judicious precautions, in excellent preservation, both contained some Greek characters, written apparently in a pretty legible hand. That which was most intelligible had appeared at first sight to contain some words relating to the service of the Christian Church.' Passing thence to speak of Casati's papyrus, Dr. Young remarks that it was the first in which any intelligible characters of the enchorial form had been discovered among the many manuscripts and inscriptions which had been examined, and it 'furnished M. Champollion with a name which materially advanced the steps leading him to his very important extension of the hieroglyphical alphabet. He had mentioned to me, in conversation, the names of Apollonius, Antiochus, and Antigonus, as occurring among the witnesses; and I easily recognised the groups which he had deciphered; although, instead of *Antiochus*, I read Antimachus; and I did not recollect at the time that he had omitted the m.'

Now comes the strange part of the story.

'In the evening of the day that Mr. Grey had brought me his manuscripts,' proceeds Dr. Young (whose English, by the way, is in places slightly questionable), 'I proceeded impatiently to examine

that which was in Greek only; and I could scarcely believe that I was awake and in my sober senses, when I observed among the names of the witnesses *Antimachus Antigenis* (*sic*); and a few lines farther back, *Portis Apollonii;* although the last word could not have been very easily deciphered without the assistance of the conjecture, which immediately occured to me, that this manuscript might perhaps be a translation of the enchorial manuscript of Casati. I found that its beginning was, "A copy of an Egyptian writing;" and I proceeded to ascertain that there were the same number of names intervening between the Greek and the Egyptian signatures that I had identified, and that the same number followed the last of them. The whole number of witnesses was sixteen in each. . . . I could not therefore but conclude,' proceeds Dr. Young, after dwelling on other points equally demonstrative of the identity of the Greek and enchorial inscriptions, 'that a most extraordinary chance had brought into my possession a document which was not very likely, in the first place, ever to have existed, still less to have been preserved uninjured, for my information, through a period of near two thousand years; but that this very extraordinary translation should have been brought safely to Europe, to England, and to me, at the very moment when it was most of all desirable to me to possess it, as the illustration of an original which I was then studying, but without any other reasonable hope of comprehending it; this combination would, in other times,

have been considered as affording ample evidence of my having become an Egyptian sorcerer.' The surprising effect of the coincidence is increased when the contents of this Egyptian manuscript are described. 'It relates to the sale, not of a house or a field, but of a portion of the collections and offerings made from time to time on account or for the benefit of a certain number of mummies of persons described at length in very bad Greek, with their children and all their households.'

The history of astronomy has in quite recent times afforded a very remarkable instance of repeated coincidences. I refer to the researches by which the theory has been established, that meteors and comets are so far associated that meteor systems travel in the track of comets. It will readily be seen from the following statements, all of which may be implicitly relied upon, that the demonstration of this theory must be regarded as partly due to singular good fortune:—

There are two very remarkable meteor systems—the system which produces the November shooting-stars, or *Leonides*, and that which produces the August shooting-stars, or *Perseides*. It chanced that the year 1866 was the time when a great display of November meteors was expected by astronomers. Hence, in the years 1865 and 1866 considerable attention was directed to the whole subject of shooting-stars. Moreover, so many astronomers watched the display of 1866, that very exact information was for the first

time obtained as to the apparent track of these meteors. It is necessary to mention that such information was *essential* to success in the main inquiry. Now it had chanced that in 1862 a fine comet had been seen, whose path approached the earth's path very closely indeed. This led the Italian astronomer Schiaparelli to inquire whether there might not be some connection between this comet and the August shooting-stars, which cross the earth's path at the same place. He was able, by comparing the path of the comet and the apparent paths of the meteors, to render this opinion highly probable. Then came inquiries into the real paths of the November meteors, these inquiries being rendered just practicable by several coincidences, as —(1) the exact observations just mentioned; (2) the existence of certain old accounts of the meteor shower; (3) the wonderful mastery obtained by Professor Adams over all problems of perturbation (for the whole question depended on the way in which the November meteors had been perturbed); and (4) the existence of a half-forgotten treatise by Gauss, supplying formulæ which reduced Adams' labour by one-half. The path having been determined (by Adams alone, I take this opportunity of insisting),* the whole question rested on the recognition of a comet travelling in the same path. If such a comet were

* Leverrier, Schiaparelli, and others calculated the path on the assumption that the occurrence of displays three times per century implies a periodic circulation around the sun in about thirty-three years and a quarter; but Adams alone proved that this period, and no other, must be that of the November meteors.

found, Schiaparelli's case was made out. If not, then, though the evidence might be convincing to mathematicians well grounded in the theory of probabilities, yet it was all but certain that Schiaparelli's theory would presently sink into oblivion. Now there are probably hundreds of comets which have a period of thirty-three and a quarter years, but very few are known—only three certainly—and one of these *had only just been discovered* when Adams' results were announced. The odds were enormous against the required comet being known, and yet greater against its having been so well watched that its true path had been ascertained. Yet the comet which had been discovered in that very year 1866—the comet called Tempel's, or I. 1866—was the very comet required to establish Schiaparelli's theory. *There* was the path of the meteors assigned by Adams, and the path of the comet had been already calculated by Tempel before Adams' result had been announced; and these two paths were found to be to all intents and purposes (with an accuracy far exceeding indeed the requirements of the case) *identical.*

To the remarkable coincidences here noted, coincidences rendered so much the more remarkable by the fact that the August comet is now known to return only twice in three centuries, while the November comet returns only thrice per century, may be added these:—

The comet of 1862 was observed, telescopically, by Sir John Herschel under remarkably favourable circum-

stances. 'It passed us closely and swiftly,' says Herschel, 'swelling into importance, and dying away with unusual rapidity. The phenomena exhibited by its nucleus and head were on this account peculiarly interesting and instructive, *it being only on very rare occasions that a comet can be closely inspected at the very crisis of its fate, so that we can witness the actual effect of the sun's rays on it.*' (This was written long before Schiaparelli's theory had attracted notice). This comet was also the last observed and studied by Sir John Herschel. The November comet, again, was the *first comet ever analysed with the spectroscope.*

It will be remarked, perhaps that where coincidences so remarkable as these are seen to be possible, it may be questionable whether the theory itself, which is based on the coincidence of certain paths, can be accepted as trustworthy. It is to be noticed that, whether this be so or not, the surprising nature of the coincidence is in no way affected; it would be as remarkable (at least) that so many events should concur to establish a false as to establish a true theory. This noted, we may admit that in this case, as in many others, the evidence for a scientific theory amounts in reality only to extreme probability. However, it is to be noticed that the probability for the theory belongs to a higher *order* than the probability against those observed coincidences which rendered the demonstration of the theory possible. The odds were thousands to one, perhaps, against the occurrence of these coincidences: but they are millions to one against the

coincidence of the paths as well of the November as of the August meteors with the paths of known comets, by mere accident.

It may possibly be considered that the circumstances of the two last cases are not altogether such as to assure us that special intervention was not in question in each instance. Indeed, though astronomers have not recognised anything snpernatural in the series of events which led to the recognition of the association between meteors and comets, some students of archæology have been disposed to regard the events narrated by Dr. Young as strictly providential dispensations. 'It seems to the reflective mind,' says the author of the *Ruins of Sacred and Historic Lands*, 'that the appointed time had at length arrived when the secrets of Egyptian history were at length to be revealed, and to cast their reflective light on the darker pages of sacred and profane history . . . The incident in the labours of Dr. Young seems so surprising that it might be deemed providential, if not miraculous.' The same will scarcely be thought of such events (and their name is legion) as De Morgan has recorded; since it requires a considerable stretch of imagination to conceive that either the discovery of the name of a certain editor, or the removal of De Morgan's difficulties respecting the siege of Boston, was a *nodus* worthy of miraculous interposition.

For absolute triviality, however, combined with singularity of coincidence, a circumstance which occurred to me several years ago appears unsurpassable. I was

raising a tumbler in such a way that at the moment it was a few inches above my mouth; but whether to examine its substance against the light, or for what particular purpose, has escaped my recollection. Be that as it may, the tumbler slipped from my fingers and fell so that the edge struck against one of my lower teeth. The fall was just enough to have broken the tumbler (at least, against a sharp object like a tooth), and I expected to have my mouth unpleasantly filled with glass fragments and perhaps seriously cut. However, though there was a sharp blow, the glass remained unbroken. On examining it, I found that a large drop of wax had fallen on the edge at the very spot where it had struck my tooth, an indentation being left by the tooth. Doubtless the softening of the shock by the interposition of the wax had just saved the glass from fracture. In any case, however, the surprising nature of the coincidence is not affected. On considering the matter it will be seen how enormous were the antecedent odds against the observed event. It is not an usual thing for a tumbler to slip in such a way: it has not at any other time happened to me, and probably not a single reader of these lines can recall such an occurrence either in his own experience or that of others. Then it very seldom happens, I suppose, that a drop of wax falls on the edge of a tumbler and there remains unnoticed. That two events so unusual should be coincident, and that the very spot where the glass struck the tooth should be the place where the wax had fallen, certainly seems

most surprising. In fact, it is only the utter triviality of the whole occurrence which renders it credible; it is just one of those events which no one would think of inventing. Whether credible or not, it happened. As De Morgan says of the coincidences he relates, so can I say for the above (equally important) circumstance, 'I can solemnly vouch for its literal truth.' Yet it would be preposterous to say that there was anything providential in such an occurrence. Swift, in his *Tale of a Tub*, has indicated in forcible terms the absurdity of recognising miraculous interventions in such cases; but should it appear to some of my readers that, trivial though the event was, I should have recognised the hand of Providence in it, I would remark that it requires some degree of self-conceit to regard oneself as the subject of the special intervention of Providence, and moreover that Providence might have contrived the escape in less complicated sort by simply so arranging matters that the glass had not fallen at all. So, at least, it appears to me.

There arises, in certain cases, the question whether coincidences may not appear so surprising as to justify the assumption that they are due to a real though undiscerned association between the coinciding events. This, of course, is the very basis of the scientific method; and it is well to notice how far this method may sometimes be unsafe. If remarkable coincidences can occur when there is no real connection—as we have seen to be the case—caution must be required in

recognising coincidence as demonstrative of association.

Not to take any more scientific instances, of which perhaps I have already said enough, let us consider the case of presentiments of death or misfortune. Here, in the first place, the coincidences which have been recorded are not so remarkable as might at first sight appear, simply because such presentiments are very common indeed. A certain not unusual condition of health, the pressure of not uncommon difficulties or dangers, depression arising from atmospheric and other causes, many circumstances, in fact, may suggest (and do notoriously suggest) such presentiments. That some presentiments out of very many thus arising should be fulfilled is not to be regarded as surprising—on the contrary, the reverse would be very remarkable. But again a presentiment may be founded on facts, known to the person concerned, which may fully justify the presentiment. 'Sometimes,' says De Morgan on this point, 'there is no mystery to those who have the clue.' He cites instances. 'In the *Gentleman's Magazine* (vol. 80, part 2, p. 33) we read, the subject being presentiment of death, as follows:—"In 1718, to come nearer the recollection of survivors, at the taking of Pondicherry, Captain John Fletcher, Captain De Morgan"' (De Morgan's grandfather) '"and Lieutenant Bosanquet each distinctly foretold his own death on the morning of his fate.' I have no doubt of all three; and I knew it of my grandfather long before I read the above passage. He saw that the battery he

commanded was unduly exposed—I think by the sap running through the fort when produced.* He represented this to the engineer officers, and to the commander-in-chief; the engineers denied the truth of the statement, the commander believed them, my grandfather quietly observed that he must make his will, and the French fulfilled the prediction. His will bore date the day of his death; and I always thought it more remarkable than the fulfilment of his prophecy that a soldier should not consider any danger short of one like the above sufficient reason to make his will. 'I suppose,' proceeds De Morgan, 'the other officers were similarly posted. I am told that military men very often defer making their wills until just before an action; but to face the ordinary risks intestate, and to wait until speedy death must be the all but certain consequence of a stupid mistake, is carrying the principle very far.'

As to the fulfilment of dreams and omens, it is to be noticed that many of the stories bearing on this subject fail in showing that the dream was fully described *before* the event occurred which appeared to fulfil the dream. It is not unlikely that if this had been done, the fulfilment, in many cases, would not have appeared quite so remarkable as in the actual narrative. Without imputing untruth to the dreamer, we may nevertheless—merely by considering what is

* De Morgan writes somewhat inexactly here for a mathematician. The sap did not run through the fort, but the direction of the sap so ran.

known as to ordinary testimony—believe that the occurrences of the dream have been somewhat modified after the event. I do not doubt that if every person who had a dream leaving a strong impression on the mind, were at once to record all the circumstances of the dream, very striking instances of fulfilment would occur before long; but at present, certainly, nine-tenths of the remarkable stories about dreams fail in the point I have referred to.

The great objection, however, to the theory that certain dreams have been intended to foreshadow real events, is the circumstance that the instances of fulfilment are related, while the instances of non-fulfilment are forgotten. It is known that instances of the latter sort are very numerous, but what proportion they bear to instances of the former sort, is unknown; and while this is the case, it is impossible to form any sound opinion on the subject, so far as actual evidence is concerned. It must be remembered that in this case we are not dealing with a theory which will be disposed of if one undoubted negative instance be adduced. It is very difficult to draw the line between dreams of an impressive nature—such dreams as we might conceive to be sent by way of warning—and dreams not specially calculated to attract the dreamer's attention. A dream which appeared impressive when it occurred but was not fulfilled by the event, would be readily regarded, even by the dreamer himself, as not intended to convey any warning as to the future. The only way to form a just opinion would be to

record each dream of an impressive nature, immediately after its occurrence, and to compare the number of cases in which such dreams are fulfilled with the number in which there is no fulfilment. Let us suppose that a certain class of dreams were selected for this purpose. Thus, let a society be formed, every member of which undertakes that whenever on the night preceding a journey he dreams of misfortune on the route, he will record his dream, with his ideas as to its impressiveness, before starting on his journey. A great number of such cases would soon be collected, and we may be sure that there would be several striking fulfilments, and probably two or three highly remarkable cases of the sort; but for my own part, I strongly entertain the opinion that the percentage of fulfilments would correspond very closely with the percentage due to the common risks of travelling, with or without premonitory dreams. This could readily be tested, if the members of the society agreed to note every occasion on which they travelled: it would be found, I suspect, that the dreamers gained little by their warnings. Suppose, for instance, that ten thousand journeys of all sorts were undertaken by the members of the society in the course of ten years, and that a hundred of these journeys (one per cent., that is) were unfortunate; then, if one-tenth of the journeys (a thousand in all) were preceded by warning dreams, I conceive that about ten of these warnings (or one per cent.) would be fulfilled. If more were fulfilled there would appear, so far as the evidence went, to be a

balance of meaning in the warnings; if fewer, it would appear that warning dreams were to some slight degree to be interpreted by the rule of contraries; but if about the proper average number of ill-omened voyages turned out unfortunately, it would follow that warning dreams had no significance or value whatever: and this is precisely the result I should expect.

Similar reasoning, and perhaps a similar method, might be applied to cases where the death of a person has been seemingly communicated to a friend or relative at a distance, whether in a dream or vision, or in some other way at the very instant of its occurrence. It is not, however, by any means so clear that in such instances we may not have to deal with phenomena admitting of physical interpretation. This is suggested, in fact, by the application of considerations resembling those which lead to the rejection of the belief in dreams giving warning against dangers. Dreams of death may indeed be sufficiently common, and but little stress could be laid, therefore, on the fulfilment of several or even of many such dreams. But visions of the absent are not common phenomena. That state of the health which occasions the appearance of visions is unusual; and if some of the stories of death-warnings are to be believed, visions of the absent have appeared to persons in good health. But setting aside the question of health, visions are unusual phenomena. Hence, if any considerable proportion of those narratives be true, which relate how a person has at the moment of his death appeared in a vision to

some friend at a distance, we must recognize the possibility, at least, that under certain conditions mind may act on mind independently of distance. The *à priori* objections to this belief are, indeed, very serious, but *à priori* reasoning does not amount to demonstration. We do not *know* that even when under ordinary circumstances we think of an absent friend, his mind may not respond in some degree to our thoughts, or else that our thoughts may not be a response to thoughts in his mind. It is certain that such a law of thought might exist and remain undetected—it would indeed be scarcely detectible. At any rate, we know too little respecting the mind to be certain that no such law exists. If it existed, then it is quite conceivable that the action of the mind in the hour of death might raise a vision in the mind of another.

I shall venture to quote here an old but well-authenticated story, as given by Mr. Owen in his *Debatable Land between this World and the Next*, leaving to my readers the inquiry whether probabilities are more in favour of the theory that (1) the story is untrue, or (2) the event related was only a remarkable coincidence between a certain event and a certain cerebral phenomenon, in reality no way associated with it, or (3) that there was a real association physically explicable, or (4) that the event was supernatural. Lord Erskine related to Lady Morgan — herself a perfect sceptic—(I wish, all the same, that the story came direct from Erskine) the following personal narrative:—'On arriving at Edinburgh one morning,

after a considerable absence from Scotland, he met in the street his father's old butler, looking very pale and wan. He asked him what brought him to Edinburgh. The butler replied, "To meet your honour, and solicit your interference with my lord to recover a sum due to me, which the steward at the last settlement did not pay." Lord Erskine then told the butler to step with him into a bookseller's shop close by, but on turning round again he was not to be seen. Puzzled at this he found out the man's wife, who lived in Edinburgh, when he learnt for the first time that the butler was dead, and that he had told his wife, on his death-bed, that the steward had wronged him of some money, and that when Master Tom returned he would see her righted. This Lord Erskine promised to do, and shortly afterwards kept his promise.' Lady Morgan then says, 'Either Lord Erskine did or did not believe this strange story: if he did, what a strange aberration of intellect! if he did not, what a stranger aberration from truth! My opinion is that he *did* believe it.' Mr. Owen deals with the hypothesis that aberration of intellect was in question, and gives several excellent reasons for rejecting that hypothesis; and he arrives at the conclusion that the butler's phantom had really appeared after his death. 'The natural inference from the facts, if they are admitted, is,' he says, 'that under certain circumstances, which as yet we may be unable to define, those over whom the death-change has passed, still interested in the concerns of earth, may for a time at least retain the power of occasional

interference in these concerns; for example, in an effort to right injustice done.' He thus adopts what, for want of a better word, may be called the supernatural interpretation. But it does not appear from the narrative (assuming it to be true) that the butler was dead at the moment when Erskine saw the vision and heard the words. If this moment preceded the moment of the butler's death, the story falls into the category of those which seem explicable by the theory of brain-waves. I express no opinion.

I had intended to pass to the consideration of those appearances which have been regarded as ghosts of departed persons, and to the study of some other matters which either are or may be referred to coincidences and superstitions. But my space is exhausted. Perhaps I may hereafter have an opportunity of returning to the subject—not to dogmatize upon it, nor to undertake to explain away the difficulties which surround it, but to indicate the considerations which, as it appears to me, should be applied to the investigation of such matters, by those who wish to give a reason for the belief that is in them.

At present I must be content with indicating the general interpretation of coincidences which appear very remarkable, but which nevertheless cannot be reasonably referred to special interpositions of Providence. The fact really is that occasions are continually occurring where coincidences of the sort are *possible*, though improbable. Now the improbability in any particular case would be a reasonable ground for

expecting that in that case no coincidence would occur. But the matter is reversed when a great multitude of cases are in question. The probable result then is that there *will* be coincidences. This may easily be illustrated by reference to a question of ordinary probabilities. Suppose there is a lottery with a thousand tickets and but one prize. Then it is exceedingly unlikely that any particular ticket-holder will obtain the prize—the odds are, in fact, 999 to 1 against him. But suppose he had one ticket in each of a million different lotteries all giving the same chance of success. Then it would not be surprising for him to draw a prize; on the contrary, it would be a most remarkable coincidence if he did not draw one. The same event—the drawing of a prize—which in one case must be regarded as highly improbable, becomes in the other case highly probable. So it is with coincidences which appear utterly improbable. It would be a most wonderful thing if such coincidences did not occur, and occur pretty frequently, in the experience of every man, since the opportunities for their occurrence enormously outnumber the chances against the occurrence of any particular instance.

We may reason in like manner as to superstitions. Or rather, it is to be noted that the coincidences on which superstitions are commonly based are in many instances not even remarkable. Misfortunes are not so uncommon, for instance, that the occurrence of a disaster of some sort after the spilling of salt at table can be regarded as surprising. If three or four persons,

who are discussing the particular superstition relating to salt-cellars, can cite instances of an apparent connection between a misfortune and the contact of salt with a table-cloth, the circumstance is in no sense to be wondered at; it would be much more remarkable if the contrary were the case. There is scarcely a superstition of the commoner sort which is not in like manner based, *not* on some remarkable coincidence, but on the occasional occurrence of quite common coincidences. It may be said, indeed, of the facts on which nearly all the vulgar superstitions have been based, that it would have amounted to little less than a miracle if such facts were not common in the experience of every person. Any other superstitions could be just as readily started, and be very quickly supported by as convincing evidence. If I were to announce to-morrow in all the papers and on every wall that misfortune is sure to follow when any person is ill-advised enough to pare a finger-nail between ten and eleven o'clock on any Friday morning, that announcement would be supported within a week by evidence of the most striking kind. In less than a month it would be an established superstition. If this appears absurd and incredible, let the reader consider merely the absurdity of ordinary superstitions. Take, for instance, fortune-telling by means of cards. If our police reports did not assure us that such vaticination is believed in by many, would it be credible that reasoning beings could hope to learn anything of the future from the order in which a

few pieces of painted paper happened to fall when shuffled? Yet it is easy to see why this or any way of telling fortunes is believed in. Many persons believe in the predictions of fortune-tellers for the seemingly excellent reason that such predictions are repeatedly fulfilled. They do not notice that (setting apart happy guesses based on known facts) there would be as many fulfilments if every prediction had been precisely reversed. It is the same with other common superstitions. Reverse them, and they are as trustworthy as before. Let the superstition be that to every one spilling salt at dinner some great piece of good luck will occur before the day is over; let seven years of good fortune be promised to the person who breaks a mirror; and so on. These new superstitions would be before long supported by as good evidence as those now in existence; and they would be worth as much, since neither would be worth anything.

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NOTES ON GHOSTS AND GOBLINS.

THERE are few subjects more perplexing, on a close examination, than the ideas of men about the supernatural (as distinguished from the religious). Whether we analyse particular superstitions and endeavour to understand what is actually believed respecting them, or whether, taking a wider view, we consider the origin

of the widespread belief in supernatural agencies, we find ourselves beset with difficulties; and these are only preliminary to the great difficulty of all—that of determining how far it is reasonable or likely that any of the common ideas about the supernatural have any basis of fact whatever.

But the first difficulty to be encountered resides in oneself. I who write have my superstitions. If I simply had them and believed in them, there would be little difficulty. But I do not believe in them. I know that they exist, because on certain occasions I have felt them in operation. Every reader of these lines must have had similar experiences—vague terrors coming we know not whence, and refusing to be exorcised by reason; the feeling—not momentary though transient—that a sight or sound is not of this world; and other sensations conveying to us a sense of the supernatural which we can neither analyse nor understand, and in which the reason has no real belief.

Perhaps the consideration of this very difficulty may throw some light on our subject, for it often happens that the key to an enigma is indicated by the more perplexing circumstances of the problem. If we dismiss for the moment all those superstitions which may fairly be regarded as derived from early impressions, or as resulting from mere ignorance, and consider the case of well-educated, carefully trained, and not weak-minded persons, who nevertheless at times experience superstitious tremors, we may perhaps find some cir-

cumstances pointing to the very origin of the superstitions now so widely entertained.

One well-marked feature of these emotions is their occurrence in the hours of darkness. I am not speaking here of the feeling of discomfort and fear which many experience when in the dark. This feeling is itself well worth inquiring into. But I now speak of the circumstance that even those who have no unpleasant sensations when in darkness, are nevertheless only exposed to certain emotions of superstitious terror at such times. Who, for instance, thoroughly enjoys a ghost story if it is told in a well-lighted room? I use the word 'enjoy,' because, as a matter of fact, the sensation I am now considering is not by any means a painful one, except in extreme cases, or with persons of weak nerves. It is a mysterious, indefinable thrill, with about the same proportion of pain and pleasure as in the feeling of melancholy experienced on certain still, bright days in spring; and it is as difficult to understand why darkness and stillness should be essential to one feeling as why brightness and stillness should be essential to the other.

There is a commonplace explanation which ascribes both these feelings to the unconscious recalling of the emotions of childhood. To the child darkness conveys the idea of discomfort. All that is enjoyable to him after darkness has come on, is in the light and warmth of the room where he sits or plays. Cold and gloom are without—in the long passages, in the unused rooms, and, in a yet greater degree, outside the house.

The childish mind finds, indeed, a strange significance in the words 'the outer darkness.' Now, one can understand that any circumstances recalling those feelings of childhood would bring with them a thrill, relieved from pain because reason tells us no real danger is present, and conveying something of pleasure much as the idea of warmth and comfort is suggested by the roar of distant winds, or the sound of rain, when we are sitting in a cozy room. And in like manner one can understand how a bright still day in spring may bring back 'in sweet and bitter fancy' the feelings of childhood.

Yet there is more in either sensation than the mere unconscious remembrance of childhood. Something much farther back in our natures, if I may so speak, is touched, when the soul thrills with unintelligible fears. The proof of this is found in the fact that the feeling exists in childhood—nay, is more marked among children than with grown persons. 'This kind of fear,' says Charles Lamb, who knew better than most men what it is, 'predominates in the period of sinless infancy.' And I think that in the same essay he touches the real solution of the mystery, or rather he presents that higher mystery from which this one takes its origin, when he says, 'these terrors are of older standing—they date beyond body.'

There is a curious story in Darwin's latest work, which he uses as an illustration of a theory yet more singular. 'My daughter,' he says, 'poured some water into a glass close to the head of a kitten, and it

immediately shook its feet.' 'It is well known,' he had before said, 'that cats dislike wetting their feet, owing, it is probable, to their having aboriginally inhabited the dry country of Egypt.' This explanation may not be the true one; but even if not, the real explanation we may be sure is quite as singular. Now the fact to be explained is analogous to the circumstance we are dealing with. We see in young creatures, like kittens, habits which cannot have been acquired from observation. These habits depend (almost certainly) on inherited peculiarities of the brain's conformation. May it not be that it is so with the superstitious tremors we have been considering? Those fears which affect children too young to know what fear is, those fears which in after life are but partially under the control of reason, may indicate a condition of the brain inherited not from parents or grandparents, but through long lines of descent—even, perhaps, from the ages when to our savage progenitors every unexplained sight or sound might indicate the presence of a lurking enemy. During long ages of savage life the conformation of the brain must have become permanently affected by the mental action resulting from the necessity for continual watchfulness against brute and human enemies. In the dark, particularly, such watchfulness was at once more requisite and more difficult; and it seems by no means unlikely that the anxious feelings which many experience constantly in the dark, as well as those peculiar tremors which are occasionally experienced in the

hours of darkness, depend on mental peculiarities inherited from our gloom-fearing savage ancestors.

As respects the ordinary feeling of dread in darkness, although there can be no doubt that it is sometimes engendered by the talk of foolish nurses to young children (and, by the way, what an unhappy thing it is that so many must pass through the mischievous ordeal of training by foolish and ignorant persons), yet it is a mistake to suppose that this is the sole or even the main cause. Some children fear to be in darkness who have never heard of ghost or goblin. 'It is not book or picture,' says Lamb very justly, 'or the stories of foolish servants, which create these terrors in children. They can at most but give them a direction. Dear little T. H., who of all children has been brought up with the most scrupulous exclusion of every taint of superstition—who was never allowed to hear of goblin or apparition, or scarcely to be told of bad men, or to read or hear of any distressing story—finds all this world of fear, from which he has been so rigidly excluded, *ab extrâ*, in his own 'thick-coming fancies;' and from his little midnight pillow, this nurse-child of optimism will start at shapes unborrowed of tradition, in sweats to which the reveries of the cell-damned murderer are tranquillity. Gorgons and Hydras and Chimæras dire—stories of Celæno and the Harpies—may reproduce themselves in the brain of superstition; but they were there before. They are transcripts, types—the archetypes are in us, and eternal.'

Another remarkable circumstance in the superstitious

impressions which affect those who have no real belief in ghosts and goblins, is the singular intensity of such impressions when aroused (in whatever way) immediately on waking. Especially after dreaming, when the dream has been of an impressive nature, the mind seems exposed to ideas of the supernatural. One often finds it impossible to understand, on waking again in full daylight, how the mind can possibly have entertained the feelings which had made night hideous or distressing. In remembrance, the matter seems like an experience of another person.

In passing it may be noticed that we perhaps owe to dreams many of the common ideas about spiritual agencies. Mr. Herbert Spencer accounts for the earliest belief in the supernatural 'by man being led through dreams, shadows, and other causes, to look at himself as a double essence, corporeal and spiritual.' And 'the spiritual being is supposed to exist after death, and to be powerful.' Mr. Tylor also has shown how dreams may have given rise to the notion of spirits; 'for savages,' says Darwin (stating Tylor's views), 'do not readily distinguish between subjective and objective impressions. When a savage dreams, the figures which appear before him are believed to have come from a distance, and to stand over him, or, "the soul of the dreamer goes out on its travels, and comes home with a remembrance of what it has seen."' 'Nevertheless,' says Darwin presently, 'I cannot but suspect that there is a still earlier and ruder stage, when anything which manifests power or move-

ment is thought to be endowed with some form of life, and with mental faculties analagous to our own.'

Another circumstance which seems to have considerable effect in preparing the mind to entertain superstitious emotions is intense or long-continued brooding on sorrows, and especially on the loss of one dear to us. Mingled with our thoughts at such times, the idea is always more or less conscientiously entertained that our lately-lost friend is near to us and knows our thoughts. The reason may be convinced

> No spirit ever brake the band
> That stays him from his native land,
> Where first he walk'd when claspt in clay;

while nevertheless something within us teaches (wrongly or rightly, who knows?) that the spirit itself

> May come
> When all the nerve of sense is numb,
> Spirit to spirit, ghost to ghost.

Surely it is not the weak and ignorant alone who have this experience. The mind of strongest mould need not be ashamed to have entertained the thought, to have even prayed the prayer,—

> Descend, and touch, and enter; hear
> The wish, too strong for words to name,
> That in this blindness of the frame
> My Ghost may feel that thine is near.

Under the influence of emotions such as these the mind is prepared to be deceived. It is at such times that visions of the departed have been seen. I do not here speak of visions called up out of nothing—the healthy mind cannot be so far betrayed—but of visions none the less imaginary. The mind has no creative

power to *form* such visions, except when there is diseased and abnormal action; but it possesses a power to combine real objects so as to form pictures of the unreal, and this power is singularly active in the time of sorrowing for a near and dear friend.

It is probable that the experience of every reader of these lines will supply instances in point. Sometimes the deception of the mind is singularly complete, insomuch that it is only by the determination to approach the seeming vision that the ghost-seer is able to remove the impression. I will cite an instance which occurred to myself, as somewhat aptly illustrating the principal circumstances tending to make such illusions effective:—

My mother died during the long vacation of my first year at Cambridge. It chanced that I was in Germany at the time, and I suffered much distress of mind from the thought that I had been enjoying a pleasure-tour during the days of her last illness. Letters had followed me from place to place, but it was only the circumstance of my staying my journey one Sunday at Heidelberg which enabled me to receive news from England; and I only reached home in time to attend her funeral. Yet the full effect of these circumstances was only experienced when I found myself again settled in my rooms at Cambridge. There is a singular mixture of society and solitude in university life, which at times of trouble produces unpleasant feelings. Throughout the day there is abundant opportunity for intercourse with friends; but although amongst one's

college friends are some who will be friends for life, yet at the time the interchange of ideas even with these special friends relates almost wholly to college work or college interests. There is nothing homelike in social arrangements at college. So soon as the 'oak is sported' for the evening a lonely feeling is apt to come on, which affects even some of those who have no recent sorrows to brood over. There is a refuge in hard reading. But hard reading, in my case, had come to an end on my mother's death. I had so far accustomed myself to associate college successes with the idea of pleasure given to her that I now looked with aversion on my former studies. They could no longer gain the prize I had alone cared for. I ought, no doubt, to have had quite other feelings, but I speak of the effects I actually experienced. Now, whether the breaking up of my old plans for work had upset me, or in whatever way it happened, I certainly had never found college life so lonely and unpleasant as during the first term of my second year. And it seems to me likely that the low spirits from which I then suffered may have had something to do with the singular instance of self-deception I have now to relate :—

I had on one evening been particularly, I may say unreasonably, low-spirited. I had sat brooding for hours over dismal thoughts. These thoughts had followed me to bed, and I went to sleep still under their influence. I cannot remember my dreams—I did dream, and my dreams were melancholy—but

although I had a perfectly clear remembrance of their tenour on first waking,* they had passed altogether from my recollection the next morning. It is to be noted, however, that I was under the influence of sorrowful dreams when I awoke. At this time the light of a waning moon was shining into the room. I opened my eyes, and saw, without surprise or any conscious feeling of fear,—my mother standing at the foot of the bed. She was not 'in her habit as she lived,' but 'clothed in white samite, mystic, wonderful.' Her face was pale, though not with the pallor of life; her expression sorrowful, and tears which glistened in the moonlight stood in her eyes. And now a strange mental condition followed. My reason told me that I was deceived by appearances; that the figure I saw

* One of the most singular facts connected with the condition of the brain during and directly after sleep, is this, that although on waking one may recollect every circumstance of a dream, and even go carefully over the events of the dream with the express object of impressing them on the mind, yet if one sleeps again the whole seems, on our next waking, to have vanished completely from the memory. One can barely remember the circumstance that there had been the desire to retain the recollection of the dream. I doubt even whether this is not generally forgotten; so that in fact in most cases, there is nothing to recall either the dream or the first waking thoughts concerning it. There is a story of a person who solved a mathematical problem in his sleep, and found the solution written out on his desk, yet had no recollection of having left his bed for the purpose. Something similar once occurred to myself; but I could just recall the circumstance that I had got up to put on paper the ideas which had occurred to me in sleep. I wish I could make the story complete by saying the solution was singularly ingenious, and so on; but truth compels me to admit that it was utter rubbish. I could not have been in the full possession of my faculties—though seemingly wide awake—when I wrote it out as something worth remembering.

was neither my mother's spirit nor an unreal vision. I felt certain I was not looking at 'a phantom of the brain which would show itself without;' and I felt equally certain that no really existent spirit was there before me. Yet the longer I looked, the more perfect appeared the picture. I racked my memory to recall any objects in my bedroom which could be mistaken for a shrouded ghost; but my memory was busy recalling the features of the dead, and my brain (against the action of my will) was tracing these features in the figure which stood before me. The deception grew more and more complete until I could have spoken aloud as to a living person. Meantime, my mind had suggested, and at once rejected, the idea of a trick played me by one of my college friends. I felt a perfect assurance that whatever it was which stood before me, it was not a breathing creature self-restrained into absolute stillness. How long I remained gazing at the figure I cannot remember; but I know that I continued steadfastly looking at it until I had assured myself that (to my mind in its probably unhealthy condition) the picture was perfect in all respects. At last I raised my head from the pillow, intending to draw nearer to the mysterious figure. But it was quite unnecessary. I had not raised my head three inches before the ghost was gone, and in its place,—or rather, not in its place, but five or six feet farther away, *hung my college surplice.* It was quite impossible to restore the illusion by resuming my former position. The mind which a moment

before had been so completely deceived, rejected completely the idea of resemblance. There was nothing even in the arrangement of the folds of the surplice to justify in the slightest degree an illusion which, nevertheless, had been perfect while it lasted. Only one feature of the apparition was accounted for. I have said that the eyes shone with tears: the explanation was rather commonplace; over my surplice I had hung a rowing belt, and the silvered buckles (partly concealed by the folds of the surplice) shone in the moonlight.

The event here narrated suggests the explanation of many ghost stories which have been related with perfect good faith. I believe the imagination only acts so as to deceive the mind completely when the latter has been painfully affected and is in an unhealthy condition. When this is the case, and a vision of some departed friend is conjured up out of realities indistinctly seen, the effect on the mind will depend greatly on the ideas entertained by the victim of the illusion on the subject of ghosts and visions generally. A believer in ghosts will be too startled to inquire further. If (as happens in many instances of the kind) he can retreat from the dread presence, he will commonly do so, and remain satisfied ever after that *he* at least has 'seen a ghost.' And in this way, I doubt little, many veracious persons have been led to give their evidence in favour of the common notions about ghosts and visions.

It is a singular circumstance, however, that some-

times several persons may be deceived by an illusion such as I have been considering. There is an instance of this kind in a book on the supernatural which I read many years ago. I cannot at the moment recall the name. It dealt with all forms of mental deception, mesmerism, witchcraft, necromancing, and so on. In the part relating to visions, it cited the case of Sir Walter Scott, who soon after the death of Byron and while his mind was dwelling on the painful circumstances of that event, saw in the dusk of a large room a vision of the poet which presently *resolved itself into furniture.* Then came the case I have in my thoughts. As nearly as I can remember, the story ran thus:—A gentleman who had lately lost his wife, looking out of window in the dusk of evening, saw her sitting in a garden chair. He called one of his daughters and asked her to look out into the garden. 'Why,' she said, 'mother is sitting there.' Another daughter was called, and she experienced the same illusion. Then the gentleman went out into the garden, and found that a garden-dress of his wife's had been placed over the seat in such a position as to produce the illusion which had deceived himself and his daughters.

I know of a more curious instance, where no explanation was ever obtained, simply because the deceived persons were too frightened to seek for one. In a house in Ireland a girl lay dying. Her mother and father were with her; and her five sisters were praying for her in a neighbouring room. This room was well lit, but overhead there was a skylight and the dark sky

beyond. One of the sisters looking up towards the skylight, saw there the face of her dying sister looking sorrowfully down upon them. She seized another sister by the hand and pointed to the skylight: and one after another the sisters looked where she pointed. They spoke no word; and in a few moments their father and mother called them to the room where their sister had just died; but when afterwards they talked together about what had happened that night, it was found that *they had all seen the vision of the sorrowful face.*

A remarkable circumstance in these and many other instances of supposed visions, is the utterly unreasonable nature of the supposition actually made in the mind of the ghost-seer. In the stories where a ghost appears for some useful purpose, as to show where treasure has been concealed or to reveal the misdeeds of some person still living, the mind does not reject the event as altogether unreasonable though the circumstances may be (and commonly are) sufficiently preposterous. But one can conceive no reason whatever why a departed wife and mother should make her appearance in a garden-chair on a dusky evening, and still less why the vision of a dying sister should look down through a skylight. It is singular that on this account alone the mind does not reject the illusion in such cases.

Among the most perplexing circumstances in the common belief about ghosts, are the accepted ideas about ghostly habiliments. For instance, why should so many ghosts be clothed in white? If the answer is

that grave-clothes are white, we may inquire what a ghost wants with grave-clothes? It might as well refuse to appear without a coffin. And then, many ghosts have appeared in their habit as they lived. If we inquire what is the real conception in the ghost-seer's mind as to the nature of the vision, we find a difficulty in understanding what idea is formed by the real believer in ghosts respecting the vestments in which spirits make their appearance. This is an old difficulty. In fact, it has probably occurred to every one who has thought over a ghost story. So soon as we come to the description of the ghost's vestments there is always a hitch in the story. For my own part, I must have been a very small child indeed, when I first pondered over the question,—Who made the ghost's clothes?

Of course there is no difficulty in the case of those who believe only in ghostly apparitions as phantoms of the brain. Here a distinction must be drawn. I am not speaking of those who regard such apparitions as either due to a diseased action of the brain or to the power of fancy in forming from real objects, indistinctly seen, the picture of a departed friend; but of those who look on visions of the dead as produced by supernatural impressions on the brain. Those who think that at the will of the dead a vision may be caused to appear, can of course understand that this vision would either be clothed in the garb which had been worn during life, or in grave-clothes, or in such other dress as suited the circumstances under which the vision appeared. But this view is not ordinarily adopted by those who regard

apparitions as supernatural phenomena. They commonly regard the phantom as something really existent in the place where it is apparently seen. The dead person is *there* in some form; some essential entity representing him has the power to transport itself from the place of the departed into the presence of the living. This ordinary idea of ghostly visions is aptly rendered in Hamlet's address to the ghost. He does not speak *of* it as a vision, but *to* it as something real, although not understood:—

> Be thou a spirit of health or goblin damn'd,
> Bring with thee airs from heaven or blasts from hell,
> Be thy intents wicked or charitable,
> Thou comest in such questionable* shape,
> That I will speak to thee: I'll call thee Hamlet:
> King, father, royal Dane: O, answer me!
> Let me not burst in ignorance; but tell
> Why thy canonized bones, hearsed in death,
> Have burst their cerements; why the sepulchre,
> Wherein we saw thee quietly inurn'd
> Hath oped his ponderous and marble jaws,
> To cast thee up again.

Nor does the poet shrink from investing the ghost with the garb of life. This had been already shown in the first scene. 'Such,' says Horatio, 'was the very armour he had on, when he the ambitious Norway combatted.' And now Hamlet asks—

> What may this mean,
> That thou, dead corse, again in complete steel,
> Revisit'st thus the glimpses of the moon,
> Making night hideous; and we fools of nature
> So horridly to shake our disposition
> With thoughts beyond the reaches of our souls?
> Say, why is this? Wherefore? What should we do?

* Mistakenly understood generally to signify 'doubtful.' What is meant is obviously 'a shape as of one to whom questions can be addressed.'

Again, it is curious how thoroughly the conventional idea of a ghost or goblin is associated with the thought of a shrouded face. It may be that this is partly due to the circumstance that while the imagination may quite commonly present to us the idea of a vision in all points complete except in the face, it can be but rarely that real objects are mistaken for the actual features of a deceased friend. Be this as it may, the ghost has been pictured with concealed face from time immemorial. So Flaxman draws the ghosts encountered by Ulysses in Hades, and no really fearful ghost has shown its face since the days when fear came upon Eliphaz, the Temanite, 'and trembling which made all his bones to shake; when a spirit passed before his face and the hair of his flesh stood up; and the spirit stood still; but he *could not discern the form thereof.*'

It is curious that children, when they try to frighten each other by 'making ghosts,' cover their heads. There is another singular trick they have—they make horns to their heads with their forefingers. Why should horns be regarded as peculiarly horrible? The idea can scarcely be referred to the times of our savage ancestors, for the creatures they had chiefly to fear were certainly not the horned animals. Yet the conventional devil is horned, and, moreover, 'divideth the hoof,' and is therefore a ruminating animal.* Did

* The conventional dragon is a Pterodactylian reptile. Ruskin will have it that Turner's picture of the Dragon guarding the Hesperidan apples was a mental evolution of a Saurian reptile; but Turner himself said he got the idea of his dragon at a pantomime at Drury Lane. *Utrum horum mavis accipe.* It is a wide range from the greensand to the greenroom.

our savage ancestors keep their children in order by frightening them with stories about their horned cattle? It is certain at least that among the most portentous forms known to those children must have been the oxen and goats which formed a principal feature of their surroundings.

It must be admitted that there is something particularly hideous in a long horned face. I remember an instance where the sudden appearance of such a face, or what I took to be such, caused me a degree of discomfort certainly not justified by the occasion. Singularly enough, the event belongs to the period of my life to which I have already referred; and I may as well note that at no time, either before or since, have I even for a moment (and against the will of the mind), mistaken commonplace objects for either 'spirit of health,' or 'goblin damn'd.'

During the last weeks of the long vacation already mentioned, I went alone to Blackpool, in Lancashire. There I took lodgings in a house facing the sea. My sitting-room was on the ground-floor. On a warm autumn night I was reading with the window open: but the blind was down and was waving gently to and fro in the wind. It happened that I was reading a book on demonology; moreover, I had been startled earlier in the evening by prolonged shrieks from an upper room in the house, where my landlady's sister, who was very ill, had had an hysterical fit. I had just read to the end of a long and particularly horrible narrative, when I was disturbed by the beating of the

curtain—the wind having risen somewhat—and I got up to close the window. As I turned round for the purpose the curtain rose gently and disclosed a startling object. A fearful face was there, black, long, and hideous, and surmounted by two monstrous horns. Its eyes, large and bright, gleamed horribly, and a mouth garnished with immense teeth grinned at me. Then the curtain slowly descended. But I knew the horrible thing was there. I waited, by no means comfortably, while the curtain fluttered about, showing parts of the black monster. At last it rose again so as to disclose the whole face. But the face had lost its horror for me. For *the horns were gone.* Instead of the two nearly upright horns which before had shown black and frightful against the light background of sea and sky, these were two sloped ears as unmistakably asinine as I felt myself at the moment. When I went to the window (which before I felt unable to approach) I saw that several stray donkeys were wandering through the front gardens of the row of houses to which my lodgings belonged. It is possible that the inquisitive gentleman who had looked in at my window was attracted by the flapping curtain, which he may have taken for something edible. 'If so,' I remarked to myself, 'two of your kind have been deceived to-night.'

It would be easy to fill page after page with the details of the various ideas entertained about ghosts, goblins, and demons. Such ideas extend not only to the appearance of such beings, their apparel, appurtenances, and so on, but to the noises which they make

either of themselves or by means of various supernatural objects which they are supposed to carry about with them. Thus,—

> The sheeted dead
> Did *squeak* and gibber in the Roman streets
> A little ere the mightiest Julius fell.

And it is to be noted that as ghosts commonly show no face, so few have been known to speak with full voice. This may be because the noises heard at the hours when ghosts are seen are not such as can be by any possibility mistaken for the human voice in its ordinary tones, while nevertheless an excited imagination can frame words out of the strange sounds which can be heard in almost every house in the stillness of night. This also serves to account for the notion that ghosts can clank chains, or make other dismal noises. Sounds heard at night are highly deceptive; a small noise close by is taken for a loud noise at a distance (not necessarily a very great distance); and a noise made by objects of one kind will be mistaken for noises made by objects of a different kind altogether. A friend of mind told me he had been disturbed two nights running by a sound as of an army tramping down a road which passed some 200 yards from his house; he found the third night (I had suggested an experimental test as to the place whence the sound came) that the noise was produced by a clock in the next house, the clock having been newly placed against the party wall. We all know Carlyle's story of the ghostly voice heard each evening by a low-spirited man—a voice as of one, in like doleful dumps, pro-

claiming, 'once I was hap-hap-happy, but now I am meeserable,'—and how the ghost resolved itself into a rusty kitchen-jack. There is a case of a lady who began to think herself the victim of some delusion, and perhaps threatened by approaching illness, because each night, about a quarter-of-an-hour after she had gone to bed, she heard a hideous din in the neighbourhood of her house, or else (she was uncertain which) in some distant room. The noise was in reality the slightest possible creak (within a few feet of her pillow, however), and produced by the door of a wardrobe which she closed every night just before getting into bed. This door, about a quarter-of-an-hour after being closed, recovered its position of rest, slightly beyond which it had been pushed in closing. In another case the crawling of a snail across a window produced sounds which were mistaken for the strains of loud but distant music.

It is, perhaps, not going too far to say that our modern spirits, who deal in noise-making as well as in furniture-tilting (of yet more marvellous feats I say nothing), are not unacquainted with the means by which the ear may be deceived as in the cases just considered. Some sounds said to be heard during dark *séances* suggest the suspicion.

It will be seen that the opinion to which I incline—as the best and perhaps only natural interpretation of events supposed to be supernatural—is that real sights and sounds are modified by the imagination, either excited or diseased, into seemingly supernatural occur-

rences. It does not seem to me likely that in any large proportion of recorded (and presumably veracious) ghost-stories, there has been an actual phantom of the brain. Such phantoms are sometimes seen, no doubt, and unreal voices are sometimes heard; but the condition of the brain which leads to such effects must be regarded as altogether exceptional. Certainly it is not common. On the contrary, the play of fancy by which images are formed from objects in no way connected with the picture raised in the mind is a common phenomenon. Although some minds possess the faculty more than others, few actually want it. I suppose there is not one person in a thousand who cannot see 'faces in the fire,' for instance, though to some the pictures so produced are much more vivid than to others. Dickens tells us that in travelling through a cleared region in America at night, the trees by the roadside seemed to assume the most startling resemblance to different objects—now an old man sitting in a chair, now a funeral urn, and so on. Doubtless, not every traveller along the road under the same circumstance would have found so many fanciful tree-pictures formed for him, or perhaps any formed so distinctly, as did Dickens, with his lively imagination and wealth of mind-images. Yet probably very few persons travel along a tree-covered region in the deeper dusk of evening without fancying that the trees shape themselves into strange forms of living or inanimate objects.

But the important point to be noticed is that when the mind is deeply occupied with particular thoughts,

the imagination is more likely to conjure up pictures connected with those thoughts than such random pictures as are formed when the mind is not so preoccupied. If we admit this—and I can conceive that there can be very little doubt on the point—we can dispose very readily of the argument from coincidence, advanced by those who believe that the spirits of the dead sometimes come visibly into the presence of the living. I present this argument as urged in an analogous case (that of visions at the moment of death) by a late eminent mathematician, whose belief in the possibility at least of many things which are commonly regarded as superstitions was so well known that no apology need here be made for touching on the subject. After speaking on the general subject of coincidences, De Morgan thus, in language less simple than he commonly employs, presents the argument for spectral apparitions (at the moment of the death of the person so appearing):—'The great *ghost-paradox* and its theory of coincidence will rise to the surface in the mind of everyone. But the use of the word *coincidence* is here at variance with its common meaning. When A is constantly happening, and also B, the occurrence of A and B at the same moment is the mere coincidence which may be casualty.' (That is, this is a coincidence of the common kind.) 'But the case before us is that A is constantly happening' (*here* by A, De Morgan means a death, as he explains further on, but the explanation should come in at this point) 'while B' (the spectral appearance of the person who

dies), 'when it does happen, almost always happens with A, and very rarely without it. That is to say, such is the phenomenon asserted; and all who rationally refer it to casualty affirm that B is happening very often as well as A, but that it is not thought worthy of being recorded except when A is simultaneous.' I must venture to express my dissent from this statement; it seems to me incredible that any person would, as De Morgan asserts, *rationally* affirm that spectral appearances are 'very often' seen. 'In talking of this subject,' he proceeds, 'it is necessary to put out of the question all who play fast and loose with their secret convictions: these had better give as a reason, when they feel internal pressure for explanation, that there is no weathercock at Kilve: this would do for all cases. But persons of real inquiry will see that, first, experience does not bear out the asserted frequency of the spectre, without the alleged coincidence of death; and secondly that if the crowd of purely casual spectres were so great that it is no wonder that now and then the person should have died at or near the moment, we ought to expect a much larger proportion of cases in which the spectre should come at the moment of the death of one or another of all the cluster who are closely connected with the original of the spectre.' (This is not very distinct: any wrong spectre, with or without close connection with any particular moribund, would seem to serve De Morgan's purpose in this argument equally well. He appears to insist, however, on the fact—undoubtedly such—that if spectres were com-

monly appearing, without reference to the deaths of individuals, cases would happen pretty frequently where a spectre appeared which was not that of a person then dying, but of some near relative. I feel by no means sure, however, that I have rightly caught De Morgan's meaning.) 'But this,' he proceeds, 'is, we know, almost without example. It remains then, for all who speculate at all, to look upon the asserted phenomenon, think what they may of it, the thing which is to be explained, as a *connection* in time of the death, and the simultaneous appearance of the dead. Any person the least used to the theory of probabilities will see that purely casual coincidence, the *wrong spectre* being comparatively so rare that it may be said never to occur, is not within the rational field of possibility.

I have quoted this argument because it applies equally well to the case of spectral appearances after death. The right spectre is always seen, so far as is known, and it appears always on a suitable occasion (at least, an occasion, as nearly suitable as the case permits).

It must be admitted, however, that this explanation does not cover the facts of all ghost-stories. There are some narratives which, if accepted in all their details, appear to admit of no explanation other than that which refers the events described to supernatural causes. But it must not be forgotten that these narratives have come in every instance from believers in ghosts and spirits; and without questioning the

veracity of particular narrators, we may yet not unfairly point out that it is not absolutely impossible that at some stage or other, either in the events related or in the handing down of the story, some degree of deception may have come in. Tricks *have* been played in these matters, beyond all possibility of question. Untruths *have* been told also. The person who doubts a narrative of the marvellous is not bound to say *where* he suspects that some mistake has been made, some deception practised, some statement made which is not strictly veracious. He may not wish to say, or he may even be very far from believing, that the narrator is a trifle foolish or not quite honest. He may put faith in the persons cited as authorities for the narrative; and he may even carry his faith, as well in the sense as in the honesty of the persons concerned, a step or two farther. Yet he may still find room for doubt. Or again, he may have very little faith, and very ample room for doubt, and yet may have valid reasons for not wishing to state as much. Persons who tell marvellous stories ought not to press too earnestly for their auditor's opinion. It is neither fair nor wise.

As an instance of a story which has been unwisely insisted upon by believers in the supernatural, I take the marvellous narrative of M. Bach and the old spinet. As given in outline by Professor Wallace, it runs thus:—'M. Leon Bach purchased at an old curiosity shop in Paris a very ancient but beautiful *spinet* as a present to his father (a great-grandson of

Bach, the great composer), a musical amateur. The next night the elder Bach dreamt that he saw a handsome young man, dressed in old court costume, who told him that the spinet had been given to him by his master King Henry. He then said he would play on it an air, with words composed by the King, in memory of a lady he had greatly loved; he did so, and M. Bach woke in tears, touched by the pathos of the song. He went to sleep again, and on waking in the morning was amazed to find on his bed a sheet of paper, on which were written, in very old characters, both words and music of the song he had heard in his dreams. It was said to be by Henry III., and the date inscribed on the spinet was a few years earlier. M. Bach, completely puzzled, showed the music to his friends, and among them were some spiritualists, from whom he heard, for the first time, their interpretation of the phenomena. Now comes the most wonderful part of the history. M. Bach became himself a writing medium; and through his hand was written involuntarily a statement that inside the spinet, in a secret niche near the key-board, was a parchment, nailed in the case, containing the lines written by King Henry when he gave the instrument to his musician. The four-line stanza, which it was said would be found on the parchment, was also given, and was followed by the signature—Baldazzarini. Father and son then set to work to search for this hidden scroll, and after some two hours' close examination found, in a narrow slit, a

piece of old parchment about eleven inches by three, containing, in very old writing, nearly the same words which M. Bach had written, and signed—Henry. This parchment was taken to the *Bibliothèque Impériale* and submitted to experienced antiquarians, and was pronounced to be an undoubtedly genuine autograph of Henry III.'

'This is the story,' says Prof. Wallace, and proceeds to dwell on the care with which Mr. Owen, who narrates it (in *The Debatable Land between this World and the Next*), had examined all the details. 'Not content with ascertaining these facts at first hand, and obtaining photographs of the spinet and parchment' (!) 'of both of which he gives good representations, Mr. Owen sets himself to hunt up historical confirmation of the story, and after much research and many failures, he finds that Baltasarini was an Italian musician, who came to France in 1577, and was in great favour with Henry III.; that the King was passionately attached to Marie de Cleves, who became wife of the Prince de Condé, and that several of the allusions to her in the verses corresponded to what was known of her history. Other minuter details were found to be historically accurate.' (In other words 'the bricks are alive this day to testify it; therefore, deny it not.') 'Mr. Owen also carefully discusses the nature of the evidence, the character of the persons concerned, and the possibility of deception. M. Bach is an old man of high character; and to suppose that

he suddenly and without conceivable motives planned and carried out a most elaborate and complicated imposture, is to suppose what is wholly incredible.' (That is, we must not suppose so because we cannot suppose so.) 'Mr. Owen shows further that the circumstances are such that M. Bach could not have been an impostor even had he been so inclined, and concludes by remarking, "I do not think dispassionate readers will accept such violent improbabilities. But if not, what interesting suggestions touching spirit-intercourse and spirit-identity connect themselves with this simple narrative of M. Bach's spinet!"'

Here is a story which to most readers, I venture to say, appears absurd on the face of it, suggesting not 'interesting,' but utterly ludicrous 'ideas of spirit intercourse;' yet we are to believe it, or else indicate exactly how our doubts are divided between Mr. Owen himself (who may have been somewhat misled by his evidence), the Bachs, father and son, the spiritualist friends who instructed M. Bach how to become 'a writing medium,' and so on.

Again, we are to believe all such stories unless we are prepared with an explanation of every circumstance. It seems to me that it would be as reasonable for a person who had witnessed some ingenious conjuring tricks to insist that they should be regarded as supernatural, unless his hearers were prepared to explain the exact way in which they had been managed. Indeed, the stress laid by the superstitious on

narratives such as those related by Mr. Owen, is altogether unwarrantable in the presence of all that is known about the nature and the laws of evidence. In works like Mr. Owen's the author is witness, judge, and advocate (especially advocate) in one. Those who do not agree with him have not only no power of cross-examining, but they commonly have neither time nor inclination to obtain specific evidence on their side of the question. It requires indeed some considerable degree of faith in the supernatural to undertake the deliberate examination of the evidence adduced for ghost stories—by which I mean, not the study of the story as related, but the actual questioning of the persons concerned, as well as an examination of the scene and all the circumstances of the event. Therefore I cannot see any force in the following remarks by Professor Wallace:—'How is such evidence as this,' he says, speaking of one of Owen's stories, 'refuted or explained away? Scores, and even hundreds of equally attested facts are on record, but no attempt is made to explain them. They are simply ignored, and in many cases admitted to be inexplicable.' Yet this is not quite satisfactory, as any reader of Mr. Owen's book will be inclined to admit. *Punch* once made a Yankee debtor say—

> This debt I have repudiated long ago;
> 'Tis therefore settled. Yet this Britisher
> Keeps for repayment worritting me still.

So our philosophers declare that they have long ago decided these ghost stories to be all delusions; *there-*

fore they need only be ignored; and they feel much 'worrited' that fresh evidence should be adduced, and fresh converts made, some of whom are so unreasonable as to ask for a new trial, on the ground that the former verdict was contrary to the evidence.'

All this affords excellent reason why the 'converts' should not be ridiculed for their belief; but something more to the purpose must be urged before 'the philosophers' can be expected to devote very much of their time to the inquiry suggested. It ought to be shown that the well-being of the human race is to some important degree concerned in the matter, whereas the trivial nature of all ghostly conduct hitherto recorded is admitted even by 'converts.' It ought to be shown that the principles of scientific research can be applied to this inquiry; whereas, before spirits were in vogue the contrary was absolutely the case, while it is scarcely going too far to say that even the behaviour of spirits is to be tested only by 'converts,' and in the dark. It ought, lastly, to be shown that the 'scores and even hundreds' of well-attested facts, admittedly singular, and even, let us say, admittedly inexplicable, are not more in number than the singular and *seemingly* inexplicable facts likely to occur (by mere casualty) among the millions of millions of events which are continually occurring; but this is very far from having been as yet demonstrated: on the contrary, when we consider the scores and hundreds, and even thousands of facts which, though they have been explained, yet seemed for awhile (and might

have remained for ever) inexplicable, the wonder rather is that not a few books like Mr. Owen's, but whole libraries of books, have not been filled with the records of even more singular and inexplicable events.

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